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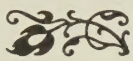

Testability and Meaning

BY

RUDOLF CARNAP

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I. INTRODUCTION

1. Our Problem: Confirmation, Testing and Meaning

TWO chief problems of the theory of knowledge are the question of meaning and the question of verification. The first question asks under what conditions a sentence has meaning, in the sense of cognitive, factual meaning. The second one asks how we get to know something, how we can find out whether a given sentence is true or false. The second question presupposes the first one. Obviously we must understand a sentence, i.e. we must know its meaning, before we can try to find out whether it is true or not. But, from the point of view of empiricism, there is a still closer connection between the two problems. In a certain sense, there is only one answer to the two questions. If we knew what it would be for a given sentence to be found true then we would know what its meaning is. And if for two sentences the conditions under which we would have to take them as true are the same, then they have the same meaning. Thus the meaning of a sentence is in a certain sense identical with the way we determine its truth or falsehood; and a sentence has meaning only if such a determination is possible.

If by verification is meant a definitive and final establishment of truth, then no (synthetic) sentence is ever verifiable, as we shall see. We can only confirm a sentence more and more. Therefore we shall speak of the problem of *confirmation* rather than of the problem of verification. We distinguish the *testing* of a sentence from its confirmation, thereby understanding a procedure—e.g. the carrying out of certain experiments—which leads to a confirmation in some degree either of the sentence itself or of its negation. We shall call a sentence *testable* if we know such a method of testing for it; and we call it *confirmable* if we know under what conditions the sentence would be confirmed. As we shall see, a sentence may be confirmable without being testable; e.g. if we know that our observation of such and such a course of events would confirm the sentence, and such and such

a different course would confirm its negation without knowing how to set up either this or that observation.

In what follows, the problems of confirmation, testing and meaning will be dealt with. After some preliminary discussions in this Introduction, a logical analysis of the chief concepts connected with confirmation and testing will be carried out in Chapter I, leading to the concept of reducibility. Chapter II contains an empirical analysis of confirmation and testing, leading to a definition of the terms 'confirmable' and 'testable' mentioned before. The difficulties in discussions of epistemological and methodological problems are, it seems, often due to a mixing up of logical and empirical questions; therefore it seems desirable to separate the two analyses as clearly as possible. Chapter III uses the concepts defined in the preceding chapters for the construction of an empiricist language, or rather a series of languages. Further, an attempt will be made to formulate the principle of empiricism in a more exact way, by stating a requirement of confirmability or testability as a criterion of meaning. Different requirements are discussed, corresponding to different restrictions of the language; the choice between them is a matter of practical decision.

2. *The Older Requirement of Verifiability*

The connection between meaning and confirmation has sometimes been formulated by the thesis that a sentence is meaningful if and only if it is verifiable, and that its meaning is the method of its verification. The historical merit of this thesis was that it called attention to the close connection between the meaning of a sentence and the way it is confirmed. This formulation thereby helped, on the one hand, to analyze the factual content of scientific sentences, and, on the other hand, to show that the sentences of trans-empirical metaphysics have no cognitive meaning. But from our present point of view, this formulation, although acceptable as a first approximation, is not quite correct. By its oversimplification, it led to a too narrow restriction of scientific language, excluding not only metaphysical sentences but also certain scientific sentences having factual meaning. Our

present task could therefore be formulated as that of a modification of the requirement of verifiability. It is a question of a modification, not of an entire rejection of that requirement. For among empiricists there seems to be full agreement that at least some more or less close relation exists between the meaning of a sentence and the way in which we may come to a verification or at least a confirmation of it.

The requirement of verifiability was first stated by *Wittgenstein*,[1] and its meaning and consequences were exhibited in the earlier publications of our *Vienna Circle*;[2] it is still held by the more conservative wing of this Circle.[3] The thesis needs both explanation and modification. What is meant by 'verifiability' must be said more clearly. And then the thesis must be modified and transformed in a certain direction.

Objections from various sides have been raised against the requirement mentioned not only by anti-empiricist metaphysicians but also by some empiricists, e.g. by *Reichenbach*,[4] *Popper*,[5] *Lewis*,[6] *Nagel*,[7] and *Stace*.[8] I believe that these criticisms are right in several respects; but on the other hand, their formulations must also be modified. The theory of confirmation and testing which will be explained in the following chapters is certainly far

[1] Wittgenstein [1].

[2] I use this geographical designation because of lack of a suitable name for the movement itself represented by this Circle. It has sometimes been called Logical Positivism, but I am afraid this name suggests too close a dependence upon the older Positivists, especially Comte and Mach. We have indeed been influenced to a considerable degree by the historical positivism, especially in the earlier stage of our development. But today we would like a more general name for our movement, comprehending the groups in other countries which have developed related views (see: Congress [1], [2]). The term '*Scientific Empiricism*' (proposed by *Morris* [1] p. 285) is perhaps suitable. In some historical remarks in the following, concerned chiefly with our original group I shall however use the term 'Vienna Circle'.

[3] Schlick [1] p. 150, and [4]; Waismann [1] p. 229.

[4] Reichenbach [1] and earlier publications; [3].

[5] Popper [1].

[6] Lewis [2] has given the most detailed analysis and criticism of the requirement of verifiability.

[7] Nagel [1].

[8] Stace [1].

from being an entirely satisfactory solution. However, by more exact formulation of the problem, it seems to me, we are led to a greater convergence with the views of the authors mentioned and with related views of other empiricist authors and groups. The points of agreement and of still existing differences will be evident from the following explanations.

A first attempt at a more detailed explanation of the thesis of verifiability has been made by *Schlick*[9] in his reply to *Lewis'* criticisms. Since 'verifiability' means 'possibility of verification' we have to answer two questions: 1) what is meant in this connection by 'possibility'? and 2) what is meant by 'verification'? Schlick—in his explanation of 'verifiability'—answers the first question, but not the second one. In his answer to the question: what is meant by 'verifiability of a sentence S', he substitutes the fact described by S for the process of verifying S. Thus he thinks e.g. that the sentence S_1: "Rivers flow up-hill," is verifiable, because it is logically possible that rivers flow up-hill. I agree with him that this fact is logically possible and that the sentence S_1 mentioned above is verifiable—or, rather, confirmable, as we prefer to say for reasons to be explained soon. But I think his reasoning which leads to this result is not quite correct. S_1 is confirmable, not because of the logical possibility of the fact described in S_1, but because of the physical possibility of the process of confirmation; it is possible to test and to confirm S_1 (or its negation) by observations of rivers with the help of survey instruments.

Except for some slight differences, e.g. the mentioned one, I am on the whole in agreement with the views of *Schlick* explained in his paper.[9] I agree with his clarification of some misunderstandings concerning positivism and so-called methodological solipsism. When I used the last term in previous publications I wished to indicate by it nothing more than the simple fact,[10] that everybody in testing any sentence empirically cannot do otherwise

[9] Schlick [4].

[10] Comp.: Erkenntnis 2, p. 461.

than refer finally to his own observations; he cannot use the results of other people's observations unless he has become acquainted with them by his own observations, e.g. by hearing or reading the other man's report. No scientist, as far as I know, denies this rather trivial fact. Since, however, the term 'methodological solipsism'—in spite of all explanations and warnings—is so often misunderstood, I shall prefer not to use it any longer. As to the fact intended, there is, I think, no disagreement among empiricists; the apparent differences are due only to the unfortunate term. A similar remark is perhaps true concerning the term 'autopsychic basis' ('eigenpsychische Basis').

Another point may be mentioned in which I do not share *Schlick's* view. He includes in the range of meaningful sentences only synthetic and analytic sentences but not contradictory ones (for an explanation of these terms see §5). In my view—and perhaps also in his—this question is not a theoretical question of truth but a practical question of decision concerning the form of the language-system, and especially the formative rules. Therefore I do not say that Schlick is wrong, but only, that I am not inclined to accept his proposal concerning the limitation of the range of sentences acknowledged as meaningful. This proposal would lead to the following consequences which seem to me to be very inconvenient. In certain cases (namely if S_1 is analytic, S_2 is contradictory, S_3 and S_4 are synthetic and incompatible with each other) the following occurs: 1) the negation of a meaningful sentence S_1 is taken as meaningless; 2) the negation of a meaningless series of symbols S_2 is taken as a meaningful sentence; 3) the conjunction of two meaningful and synthetic sentences S_3 and S_4 is taken as meaningless. By the use of technical terms of logical syntax the objection can be expressed more precisely: if we decide to include in the range of (meaningful) sentences of our language only analytic and synthetic sentences (or even only synthetic sentences,[11] then the formative rules of our language become indefinite.[12] That means that in this case we have no fixed finite method of distinguishing between the meaningful and the meaningless, i.e. between sentences and expressions which are not sentences. And this would obviously be a serious disadvantage.

[11] Comp.: Carnap [6] p. 32, 34.

[12] Comp.: Carnap [4] §45.—About the indefinite character of the concepts 'analytic' and 'contradictory' comp.: Carnap [7] p. 163, or: [4b] §34a and 34d.

3. Confirmation instead of Verification

If verification is understood as a complete and definitive establishment of truth then a universal sentence, e.g. a so-called law of physics or biology, can never be verified, a fact which has often been remarked. Even if each single instance of the law were supposed to be verifiable, the number of instances to which the law refers—e.g. the space-time-points—is infinite and therefore can never be exhausted by our observations which are always finite in number. We cannot verify the law, but we can test it by testing its single instances i.e. the particular sentences which we derive from the law and from other sentences established previously. If in the continued series of such testing experiments no negative instance is found but the number of positive instances increases then our confidence in the law will grow step by step. Thus, instead of verification, we may speak here of gradually increasing *confirmation* of the law.

Now a little reflection will lead us to the result that there is no fundamental difference between a universal sentence and a particular sentence with regard to verifiability but only a difference in degree. Take for instance the following sentence: "There is a white sheet of paper on this table." In order to ascertain whether this thing is paper, we may make a set of simple observations and then, if there still remains some doubt, we may make some physical and chemical experiments. Here as well as in the case of the law, we try to examine sentences which we infer from the sentence in question. These inferred sentences are predictions about future observations. The number of such predictions which we can derive from the sentence given is infinite; and therefore the sentence can never be completely verified. To be sure, in many cases we reach a practically sufficient certainty after a small number of positive instances, and then we stop experimenting. But there is always the theoretical possibility of continuing the series of test-observations. Therefore here also *no complete verification is possible* but only a process of gradually increasing *confirmation*. We may, if we wish, call a sentence disconfirmed[13] in a certain degree if its negation is confirmed in that degree.

[13] "Erschüttert," Neurath [6].

The impossibility of absolute verification has been pointed out and explained in detail by *Popper*.[14] In this point our present views are, it seems to me, in full accordance with *Lewis*[15] and *Nagel*.[16]

Suppose a sentence S is given, some test-observations for it have been made, and S is confirmed by them in a certain degree. Then it is a matter of practical decision whether we will consider that degree as high enough for our acceptance of S, or as low enough for our rejection of S, or as intermediate between these so that we neither accept nor reject S until further evidence will be available. Although our decision is based upon the observations made so far, nevertheless it is not uniquely determined by them. There is no general rule to determine our decision. Thus the acceptance and the rejection of a (synthetic) sentence always contains a *conventional component*. That does not mean that the decision—or, in other words, the question of truth and verification—is conventional. For, in addition to the conventional component there is always the non-conventional component—we may call it, the objective one—consisting in the observations which have been made. And it must certainly be admitted that in very many cases this objective component is present to such an overwhelming extent that the conventional component practically vanishes. For such a simple sentence as e.g. "There is a white thing on this table" the degree of confirmation, after a few observations have been made, will be so high that we practically cannot help accepting the sentence. But even in this case there remains still the theoretical possibility of denying the sentence. Thus even here it is a matter of decision or convention.

The view that no absolute verification but only gradual confirmation is possible, is sometimes formulated in this way: every sentence is a probability-sentence; e.g. by *Reichenbach*[17] and *Lewis*.[18] But it seems advisable to separate the two assertions.

[14] Popper [1].

[15] Lewis [2] p. 137, note 12: "No verification of the kind of knowledge commonly stated in propositions is ever absolutely complete and final."

[16] Nagel [1] p. 144f.

[17] Reichenbach [1].

[18] Lewis [2] p. 133.

Most empiricists today will perhaps agree with the first thesis, but the second is still a matter of dispute. It presupposes the thesis that the degree of confirmation of a hypothesis can be interpreted as the degree of probability in the strict sense which this concept has in the calculus of probability, i.e. as the limit of relative frequency. Reichenbach[19] holds this thesis. But so far he has not worked out such an interpretation in detail, and today it is still questionable whether it can be carried out at all. *Popper*[20] has explained the difficulties of such a frequency interpretation of the degree of confirmation; the chief difficulty lies in how we are to determine for a given hypothesis the series of "related" hypotheses to which the concept of frequency is to apply. It seems to me that at present it is not yet clear whether the concept of degree of confirmation can be defined satisfactorily as a quantitative concept, i.e. a magnitude having numerical values. Perhaps it is preferable to define it as a merely topological concept, i.e. by defining only the relations: "S_1 has the same (or, a higher) degree of confirmation than S_2 respectively," but in such a way that most of the pairs of sentences will be incomparable. We will use the concept in this way—without however defining it—only in our informal considerations which serve merely as a preparation for exact definitions of other terms. We shall later on define the concepts of complete and incomplete reducibility of confirmation as syntactical concepts, and those of complete and incomplete confirmability as descriptive concepts.

4. The Material and the Formal Idioms

It seems to me that there is agreement on the main points between the present views of the *Vienna Circle*, which are the basis of our following considerations, and those of *Pragmatism*, as interpreted e.g. by *Lewis*.[21] This agreement is especially marked with respect to the view that every (synthetic) sentence is a hypothesis, i.e. can never be verified completely and defini-

[19] Reichenbach [2] p. 271 ff.; [3] p. 154 ff.

[20] Popper [1] Chapter VIII; for the conventional nature of the problem compare my remark in "Erkenntnis" vol. 5, p. 292.

[21] Lewis [2], especially p. 133.

tively. One may therefore expect that the views of these two empiricist movements will continue to converge to each other in their further development; *Morris*[22] believes that this convergence is a fact and, moreover, tries to promote it.

However, in spite of this agreement on many important points, there is a difference between our method of formulation and that which is customary in other philosophical movements, especially in America and England. This difference is not as unimportant as are the differences in formulation in many other cases. For the difference in formulation depends on the difference between the material and the formal idioms.[23] The use of the material idiom is very common in philosophy; but it is a dangerous idiom, because it sometimes leads to pseudo-questions. It is therefore advisable to translate questions and assertions given in the material idiom into the formal idiom. In the material idiom occur expressions like 'facts', 'objects', 'the knowing subject', 'relation between the knowing subject and the known subject', 'the given', 'sense-data', 'experiences' etc. The formal idiom uses syntactical terms instead, i.e. terms concerning the formal structure of linguistic expressions. Let us take an example. It is a pseudo-thesis of idealism and older positivism, that a physical object (e.g. the moon) is a construction out of sense-data. Realism on the other hand asserts, that a physical object is not constructed but only cognized by the knowing subject. We—the Vienna Circle—neither affirm nor deny any of these theses, but regard them as pseudo-theses, i.e. as void of cognitive meaning. They arise from the use of the material mode, which speaks about 'the object'; it thereby leads to such pseudo-questions as the "nature of this

[22] Morris [1], [2].

[23] Here I can give no more than some rough indications concerning the material and the formal idioms. For detailed explanations compare Carnap [4]. Ch. V. A shorter and more easily understandable exposition is contained in [5] p. 85–88.—What I call the formal and the material idioms or modes of speech, is not the same as what Morris ([1], p. 8) calls the formal and the empirical modes of speech. To Morris's empirical mode belong what I call the real object-sentences; and these belong neither to the formal nor to the material mode in my sense (comp. Carnap [4], §74, and [5], p. 61). The distinction between the formal and the material idioms does not concern the usual sentences of science but chiefly those of philosophy, especially those of epistemology or methodology.

object", and especially as to whether it is a mere construction or not. The formulation in the formal idiom is as follows: "A physical object-name (e.g. the *word* 'moon') is reducible to sense-data predicates (or perception predicates)." *Lewis*[24] seems to believe that logical positivism—the Vienna Circle—accepts the idealistic pseudo-thesis mentioned. But that is not the case. The misunderstanding can perhaps be explained as caused by an unintentional translation of our thesis from the formal into the more accustomed material idiom, whereby it is transformed into the idealistic pseudo-thesis.

The same is true concerning our thesis: "My testing of any sentence, even one which contains another man's name and a psychological predicate (e.g. "Mr. X is now cheerful"), refers back ultimately to my own observation-sentences." If we translate it into: "Your mind is nothing more than a construction which I put upon certain data of my own experience," we have the pseudo-thesis of solipsism, formulated in the material idiom. But this is not our thesis.

The formulation in the material idiom makes many epistemological sentences and questions ambiguous and unclear. Sometimes they are meant as psychological questions. In this case clearness could be obtained by a formulation in the psychological language. In other cases questions are not meant as empirical, factual questions, but as logical ones. In this case they ought to be formulated in the language of logical syntax. In fact, however, epistemology in the form it usually takes—including many of the publications of the Vienna Circle—is an unclear mixture of psychological and logical components. We must separate it into its two kinds of components if we wish to come to clear, unambiguous concepts and questions. I must confess that I am unable to answer or even to understand many epistemological questions of the traditional kind because they are formulated in the material idiom. The following are some examples taken from customary discussions: "Are you more than one of my ideas"?, "Is the past more than the present recollection"?, "Is the future more than

[24] Lewis [2], p. 127–128.

the present experience of anticipation"?, "Is the self more than one of those ideas I call mine"?, "If a robot is exhibiting all the behavior appropriate to tooth-ache, is there a pain connected with that behavior or not"? etc.

I do not say that I have not the least understanding of these sentences. I see some possibilities of translating them into unambiguous sentences of the formal idiom. But unfortunately there are several such translations, and hence I can only make conjectures as to the intended meaning of the questions. Let me take another example. I find the following thesis[25] formulated in the material mode: "Any reality must, in order to satisfy our empirical concept of it, transcend the concept itself. A construction imposed upon given data cannot be identical with a real object; the thing itself must be more specific, and in comparison with it the construction remains abstract." As a conjecture, selected from a great number of possibilities, I venture the following translation into the formal idiom: "For any object-name and any given finite class C of sentences (or: of sentences of such and such a kind), there are always sentences containing that name such that neither their confirmation nor that of their negation is completely reducible to that of C (in syntactical terminology: there are sentences each of which is neither a consequence of C nor incompatible with C)." Our present views, by the way,—as distinguished from our previous ones—are in agreement with this thesis, provided my interpretation hits the intended meaning. The translation shows that the thesis concerns the structure of language and therefore depends upon a convention, namely the choice of the language-structure. This fact is concealed by the formulation in the material mode. There the thesis seems to be independent of the choice of language, it seems to concern a certain character which 'reality' either does or does not possess. Thus the use of the material idiom leads to a certain absolutism, namely to the neglect of the fact that the thesis is relative to the chosen language-system. The use of the formal idiom reveals that fact. And indeed our present agreement with the thesis

[25] *Lewis* [2], p. 138.

mentioned is connected with our admission of incompletely confirmable sentences, which will be explained later on.

The dangers of the material idiom were not explicitly noticed by our Vienna Circle in its earlier period. Nevertheless we used this idiom much less frequently than is customary in traditional philosophy; and when we used it, we did so in most cases in such a way that it was not difficult to find a translation into the formal idiom. However, this rather careful use was not deliberately planned, but was adopted intuitively, as it were. It seems to me that most of the formulations in the material idiom which are considered by others as being theses of ours have never been used by us. In recent years we have become increasingly aware of the disadvantages of the material idiom. Nevertheless we do not try to avoid its use completely. For sometimes its use is preferable practically, as long as this idiom is still more customary among philosophers. But perhaps there will come a time when this will no longer be the case. Perhaps some day philosophers will prefer to use the formal idiom—at least in those parts of their works which are intended to present decisive arguments rather than general preliminary explanations.

II. LOGICAL ANALYSIS OF CONFIRMATION AND TESTING

5. Some Terms and Symbols of Logic

In carrying out methodological investigations especially concerning verification, confirmation, testing etc., it is very important to distinguish clearly between logical and empirical, e.g. psychological questions. The frequent lack of such a distinction in so-called epistemological discussions has caused a great deal of ambiguity and misunderstanding. In order to make quite clear the meaning and nature of our definitions and explanations, we will separate the two kinds of definitions. In this Chapter II we are concerned with logical analysis. We shall define concepts belonging to logic, or more precisely, to logical syntax, although the choice of the concepts to be defined and of the way in which they are defined is suggested in some respects by a consideration of empirical questions—as is often the case in laying down logical

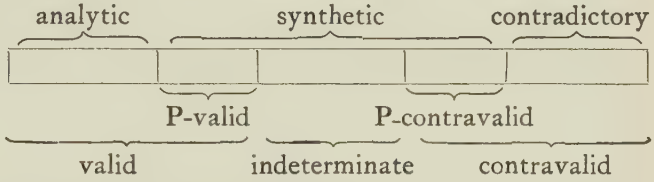
definitions. The logical concepts defined here will be applied later on, in Chapter III, in defining concepts of an empirical analysis of confirmation. These descriptive, i.e. non-logical, concepts belong to the field of biology and psychology, namely to the theory of the use of language as a special kind of human activity.

In the following logical analysis we shall make use of some few *terms of logical syntax*, which may here be explained briefly.[26] The terms refer to a language-system, say L, which is supposed to be given by a system of rules of the following two kinds. The formative rules state how to construct sentences of L out of the symbols of L. The transformative rules state how to deduce a sentence from a class of sentences, the so-called premisses, and which sentences are to be taken as true unconditionally, i.e., without reference to premisses. The transformative rules are divided into those which have a logico-mathematical nature; they are called logical rules or L-rules (this 'L-' has nothing to do with the name 'L' of the language); and those of an empirical nature, e.g. physical or biological laws stated as postulates; they are called physical rules or P-rules.

We shall take here 'S', 'S_1', 'S_2' etc. as designations of sentences (not as abbreviations for sentences). We use '$\sim$S' as designation of the negation of S. (Thus, in this connection, '$\sim$' is not a symbol of negation but a syntactical symbol, an abbreviation for the words 'the negation of'.) If a sentence S can be deduced from the sentences of a class C according to the rules of L, S is called a *consequence* of C; and moreover an L-consequence, if the L-rules are sufficient for the deduction, otherwise a P-consequence. S_1 and S_2 are called *equipollent* (with each other) if each is a consequence of the other. If S can be shown to be true on the basis of the rules of L, S is called *valid* in L; and moreover L-valid or *analytic*, if true on the basis of the L-rules alone, otherwise P-valid. If, by application of the rules of L, S can be shown to be false, S is called *contravalid*; and L-contravalid or *contradictory*, if by L-rules alone, otherwise P-contravalid. If S is neither valid

[26] For more exact explanations of these terms see Carnap [4]; some of them are explained also in [5].

nor contravalid S is called *indeterminate*. If S is neither analytic nor contradictory, in other words, if its truth or falsehood cannot be determined by logic alone, but needs reference either to P-rules or to the facts outside of language, S is called *synthetic*. Thus the totality of the sentences of L is classified in the following way:



A sentence S_1 is called incompatible with S_2 (or with a class C of sentences), if the negation $\sim S_1$ is a consequence of S_2 (or of C, respectively). The sentences of a class are called mutually independent if none of them is a consequence of, or incompatible with, any other of them.

The most important kind of predicates occurring in a language of science is that of the predicates attributed to space-time-points (or to small space-time-regions). For the sake of simplicity we shall restrict the following considerations—so far as they deal with predicates—to those of this kind. The attribution of a certain value of a physical function, e.g. of temperature, to a certain space-time-point can obviously also be expressed by a predicate of this kind. The following considerations, applied here to such predicates only, can easily be extended to descriptive terms of any other kind.

In order to be able to formulate examples in a simple and exact way we will use the following symbols. We take 'a', 'b', etc. as names of space-time-points (or of small space-time-regions), i.e. as abbreviations for quadruples of space-time-coördinates; we call them *individual constants*. 'x', 'y', etc. will be used as corresponding variables; we will call them *individual variables*. We shall use 'P', 'P_1', 'P_2' etc., and 'Q', 'Q_1' etc. as *predicates*; if no other indication is given, they are supposed to be predicates of the kind described. The sentence '$Q_1(b)$' is to mean: "The space-time-point b has the property Q_1." Such a sentence consisting

of a predicate followed by one or several individual constants as arguments, will be called a *full sentence* of that predicate.

Connective symbols: '$\sim$' for 'not' (negation), '$\vee$' for 'or' (disjunction), '$\cdot$' for 'and' (conjunction), '$\supset$' for 'if – then' (implication), '$\equiv$' for 'if – then –, and if not – then not –' (equivalence). '$\sim$Q(a)' is the negation of a full sentence of 'Q'; it is sometimes also called a full sentence of the predicate '$\sim$Q'.

Operators: '(x)P(x)' is to mean: "every point has the property P" (*universal* sentence; the first '(x)' is called the universal *operator*, and the sentential function 'P(x)' its *operand*). '$(\exists x)$P(x)' is to mean: "There is at least one point having the property P" (*existential* sentence; '$(\exists x)$' is called the existential operator and 'P(x)' its operand). (In what follows, we shall not make use of any other operators than universal and existential operators with individual variables, as described here.) In our later examples we shall use the following abbreviated notation for universal sentences of a certain form occurring very frequently. If the sentence '(x)[– – –]' is such that '– – –' consists of several partial sentences which are connected by '$\sim$', '$\vee$' etc. and each of which consists of a predicate with 'x' as argument, we allow omission of the operator and the arguments. Thus e.g. instead of '(x) $(P_1(x) \supset P_2(x))$' we shall write shortly '$P_1 \supset P_2$'; and instead of '(x) $[Q_1(x) \supset (Q_3(x) \equiv Q_2(x))]$' simply '$Q_1 \supset (Q_3 \equiv Q_2)$'. The form '$P_1 \supset P_2$' is that of the simplest physical laws; it means: "If any space-time-point has the property P_1, it has also the property P_2."

6. *Reducibility of Confirmation*

The number of sentences for which, at a certain moment, we have found a confirmation of some degree or other, is always finite. If now a class C of sentences contains a finite sub-class C′ such that the sentence S is a consequence of C′, then, if the sentences of C′ are found to be confirmed to a certain degree, S will be confirmed to at least the same degree. In this case we have, so to speak, a complete confirmation of S by C′. (It is to be noticed that "complete" is not meant here in an absolute sense, but in a relative sense with respect to certain premisses.) On the other hand,

suppose that S is not a consequence of any finite sub-class of C, but each sentence of an infinite sub-class C″ of C is a consequence of S, – e.g. if S is a universal sentence and C″ the class of its instances. In this case, no complete confirmation of S by sentences of C is possible; nevertheless, S will be confirmed by the confirmation of sentences of C″ at least to some degree, though not necessarily to the same degree. Suppose moreover that the sentences of C″ are mutually independent. Since their number is infinite, they cannot be exhausted. Therefore the degree of confirmation of S will increase by the confirmation of more and more sentences of C″ but without ever coming to a complete confirmation. On the basis of these considerations we will lay down the definitions 1 to 6. In Definitions 1 and 2 C is a class of sentences. The terms defined in Definitions 1 a, b and c are only auxiliary terms for Definition 2.

Definition 1. a. We will say that the confirmation of S is completely reducible to that of C, if S is a consequence of a finite subclass of C.

b. We will say that the confirmation of S is directly incompletely reducible to that of C, if the confirmation of S is not completely reducible to that of C but if there is an infinite sub-class C′ of C such that the sentences of C′ are mutually independent and are consequences of S.

c. We will say that the confirmation of S is directly reducible to that of C, if it is either completely reducible or directly incompletely reducible to that of C.

Definition 2. a. We will say that the *confirmation* of S is *reducible* to that of C, if there is a finite series of classes $C_1, C_2, \ldots C_n$ such that the relation of directly reducible confirmation subsists 1) between S and C_1, 2) between every sentence of C_i and C_{i+1} ($i = 1$ to $n-1$), and 3) between every sentence of C_n and C.

b. We will say that the *confirmation* of S is *incompletely reducible* to that of C, if it is reducible but not completely reducible to that of C.

Definition 3. We will say that the confirmation of S is reducible (or completely reducible, or incompletely reducible) to that of a class C of predicates (or to that of its members) if it is reduc-

ible (or completely reducible, or incompletely reducible, respectively) to a not contravalid sub-class of the class which contains the full sentences of the predicates of C and the negations of these sentences. – The sub-class is required not to be contravalid because any sentence whatever is a consequence of a contravalid class, as e.g. {'P(a)', '∼P(a)'}, and hence its confirmation is reducible to that of this class.

The following definitions concerning predicates are analogous to the previous ones concerning sentences.

Definition 4. We will say that the confirmation of a *predicate* 'Q' is reducible (or completely reducible, or incompletely reducible) to that of a class C of predicates, say 'P_1', 'P_2', etc., if the confirmation of every full sentence of 'Q' with a certain argument, e.g. 'Q(a)', is reducible (or completely reducible, or incompletely reducible, respectively) to that of the class C′ consisting of the full sentences of the predicates of C with the same argument and the negations of those sentences ('P_1(a)', '∼P_1(a)', 'P_2(a)', '∼P_2(a)', etc.).

Definition 5. A *predicate* 'Q' is called *reducible* (or completely reducible, or incompletely reducible) to a class C of predicates or to its members, if the confirmation both of 'Q' and of '∼Q' is reducible (or completely reducible, or incompletely reducible, respectively) to C.

When we speak of sentential functions, sentences are understood to be included because a sentence may be taken as a special case of a sentential function with the number zero of free variables. Therefore the following definitions are also applied to sentences.

Definition 6. A sentential function is said to have *atomic form* if it consists of one predicate followed by one or several arguments (individual constants or variables). (Examples: 'P(x)', 'Q(a, x)', 'P(a)'.

Definition 7. A sentential function is said to have *molecular form* if it is constructed out of one or several sentential functions with the help of none, one or several connective symbols (but without operators).

Definition 8. a. A sentential function is said to have *generalized form* if it contains at least one (unrestricted) operator.

b. A sentential function is said to have *essentially generalized form* if it has generalized form and cannot be transformed into a molecular form containing the same descriptive predicates.

We have to distinguish between a sentence of atomic form and an atomic sentence (see Definition 15a, §9; here the predicate occurring must fulfill certain conditions); and likewise between a sentence of molecular form and a molecular sentence (see Definition 15b, §9). Since the sentences of atomic form are included in those of molecular form, the important distinction is that between molecular and (essentially) generalized form.

In what follows we will apply the concepts of reducibility of confirmation, defined before, first to molecular sentences and then to generalized sentences.

Theorem 1. If the confirmation both of S_1 and of S_2 is completely reducible to that of a class C of predicates, then the confirmation both of their disjunction and of their conjunction is also completely reducible to that of C.

Proof. The disjunction is a consequence of S_1; the conjunction is a consequence of S_1 and S_2.

Theorem 2. If S is a sentence of *molecular form* and the descriptive predicates occurring in S belong to C, the confirmation of S is completely reducible to that of C.

Proof. Let C′ be the class of the full sentences of the predicates of C and their negations. According to a well known theorem of logic, S can be transformed into the so-called disjunctive normal form,[27] i.e. into a disjunction of conjunctions of sentences of C′. Now, the confirmation of a sentence of C′ is completely reducible to that of C. Therefore, according to Theorem 1, the confirmation of each of the conjunctions is also completely reducible to that of C, and, again according to Theorem 1, the same is true for the disjunction of these conjunctions, and hence for S.

The application of the concepts defined before to sentences of generalized form may be explained by the following examples.

S_1: $'(x)P(x)'$

S_2: $'(x) \sim P(x)'$ (in words: every point has the property not-P; in other words: no point has the property P).

[27] Compare *Hilbert* [1] p. 13.

C_1 may be taken as the class of the full sentences of 'P', i.e. the class of the particular sentences 'P(a)', 'P(b)', etc.; C_2 as the class of the negations of these sentences: '$\sim$ P(a)', etc.; and C as the sum of C_1 and C_2. Then, according to a well known result (see §3), the confirmation of S_1 is directly reducible to that of C_1 and hence to that of C, but only incompletely, because S_1 is not a consequence of any finite sub-class of C, however large this may be. On the other hand, $\sim S_1$ is a consequence of each sentence of C_2, e.g. of '$\sim$ P(a)'. Therefore the confirmation of $\sim S_1$ is completely reducible to that of C_2 and hence to that of C.

S_2 bears the same relation to C_2 as S_1 does to C_1. Therefore the confirmation of S_2 is incompletely reducible to that of C_2, and the confirmation of $\sim S_2$ is completely reducible to that of C_1. This can easily be seen when we transform $\sim S_2$ into the existential sentence '$(\exists x)P(x)$' which is a consequence of each sentence of C_1, e.g. of 'P(a)'. The results of these considerations may be exhibited by the following table which gives two formulations for each of the four sentences, one containing a universal operator and the other an existential operator. Some of the results, which we need later on, are formulated in the following Theorems 3 and 4.

	two formulations	The confirmation of S is reducible					
		to that of C_1 ('P(a)' etc.)		to that of C_2 ('$\sim$P(a)' etc.)		to that of C ($= C_1 + C_2$)	
		compl.	incompl.	compl.	incompl.	compl.	incompl.
S_1	$(x)P(x)$; $\sim (\exists x) \sim P(x)$	−	+	−	−	−	+
$\sim S_1$	$\sim (x)P(x)$; $(\exists x)(\sim P(x))$	−	−	+	−	+	−
S_2	$(x) \sim P(x)$; $\sim (\exists x)P(x)$	−	−	−	+	−	+
$\sim S_2$	$\sim (x) \sim P(x)$; $(\exists x)P(x)$	+	−	−	−	+	−

Theorem 3. Let S be the *universal sentence* '(x)P(x)'. The confirmation of S is incompletely reducible to that of the full sentences of 'P' and hence to that of 'P'. The confirmation of

$\sim$ S is completely reducible to that of the negation of any full sentence of 'P' and hence to that of 'P'.

Theorem 4. Let S be the *existential sentence* '$(\exists x)P(x)$'. The confirmation of S is completely reducible to that of any full sentence of 'P' and hence to that of 'P'. The confirmation of $\sim$ S is incompletely reducible to that of the negations of the full sentences of 'P' and hence to that of 'P'.

The Theorems 3 and 4 correspond to the following usual, but not quite correct formulations: 1) "A universal sentence is not verifiable but falsifiable," 2) "An existential sentence is verifiable but not falsifiable." Still closer corresponding theorems will be stated later on (Theorems 19 and 20, §24).

7. *Definitions*

By an (explicit) definition of a descriptive predicate 'Q' with one argument we understand a sentence of the form

(D:) $\qquad Q(x) \equiv \cdots x \cdots$

where at the place of '. . . x . . .' a sentential function – called the definiens – stands which contains 'x' as the only free variable. For several arguments the form is analogous. We will say that a definition D is based upon the class C of predicates if every descriptive symbol occurring in the definiens of D belongs to C. If the predicates of a class C are available in our language we may introduce other predicates by a chain of definitions of such a kind that each definition is based upon C and the predicates defined by previous definitions of the chain.

Definition 9. A definition is said to have atomic (or molecular, or generalized, or essentially generalized) form, if its definiens has atomic (or molecular, or generalized, or essentially generalized, respectively) form.

Theorem 5. If 'P' is defined by a definition D based upon C, 'P' is reducible to C. If D has molecular form, 'P' is completely reducible to C. If D has essentially generalized form, 'P' is incompletely reducible to C.

Proof. 'P' may be defined by '$P(x) \equiv \ldots x \ldots$'. Then, for any b, 'P(b)' is equipollent to '. . . b . . .' and hence in the case of

molecular form, according to Theorem 2, completely reducible to C, and in the other case, according to Theorems 3 and 4, reducible to C.

Let us consider the question whether the so-called *disposition-concepts* can be defined, i.e. predicates which enunciate the disposition of a point or body for reacting in such and such a way to such and such conditions, e.g. 'visible', 'smellable', 'fragile', 'tearable', 'soluble', 'indissoluble' etc. We shall see that such disposition-terms cannot be defined by means of the terms by which these conditions and reactions are described, but they can be introduced by sentences of another form. Suppose, we wish to introduce the predicate 'Q_3' meaning "soluble in water." Suppose further, that 'Q_1' and 'Q_2' are already defined in such a way that '$Q_1(x, t)$' means "the body x is placed into water at the time t," and '$Q_2(x,t)$' means "the body x dissolves at the time t." Then one might perhaps think that we could define 'soluble in water' in the following way: "x is soluble in water" is to mean "whenever x is put into water, x dissolves," in symbols:

$$(D:) \qquad Q_3(x) \equiv (t)[Q_1(x, t) \supset Q_2(x, t)].$$

But this definition would not give the intended meaning of 'Q_3'. For, suppose that c is a certain match which I completely burnt yesterday. As the match was made of wood, I can rightly assert that it was not soluble in water; hence the sentence '$Q_3(c)$' (S_1) which asserts that the match c is soluble in water, is false. But if we assume the definition D, S_1 becomes equipollent with '$(t)\ [Q_1(c, t) \supset Q_2(c,t)]$' ($S_2$). Now the match c has never been placed and on the hypothesis made never can be so placed. Thus any sentence of the form '$Q_1(c,t)$' is false for any value of 't'. Hence S_2 is true, and, because of D, S_1 also is true, in contradiction to the intended meaning of S_1. 'Q_3' cannot be defined by D, nor by any other definition. But we can introduce it by the following sentence:

$$(R:) \qquad (x)(t)[Q_1(x, t) \supset (Q_3(x) \equiv Q_2(x, t))],$$

in words: "if any thing x is put into water at any time t, then, if x is soluble in water, x dissolves at the time t, and if x is not soluble

in water, it does not." This sentence belongs to that kind of sentences which we shall call reduction sentences.

8. Reduction Sentences

Suppose, we wish to introduce a new predicate 'Q_3' into our language and state for this purpose a pair of sentences of the following form:

$$(R_1) \qquad Q_1 \supset (Q_2 \supset Q_3)$$
$$(R_2) \qquad Q_4 \supset (Q_5 \supset \sim Q_3)$$

Here, 'Q_1' and 'Q_4' may describe experimental conditions which we have to fulfill in order to find out whether or not a certain space-time-point b has the property Q_3, i.e. whether '$Q_3(b)$' or '$\sim Q_3(b)$' is true. 'Q_2' and 'Q_5' may describe possible results of the experiments. Then R_1 means: if we realize the experimental condition Q_1 then, if we find the result Q_2, the point has the property Q_3. By the help of R_1, from '$Q_1(b)$' and '$Q_2(b)$', '$Q_3(b)$' follows. R_2 means: if we satisfy the condition Q_4 and then find Q_5 the point has not the property Q_3. By the help of R_2, from '$Q_4(b)$' and '$Q_5(b)$', '$\sim Q_3(b)$' follows. We see that the sentences R_1 and R_2 tell us how we may determine whether or not the predicate 'Q_3' is to be attributed to a certain point, provided we are able to determine whether or not the four predicates 'Q_1', 'Q_2', 'Q_4'; and 'Q_5' are to be attributed to it. By the statement of R_1 and R_2 'Q_3' is reduced in a certain sense to those four predicates; therefore we shall call R_1 and R_2 reduction sentences for 'Q_3' and '$\sim Q_3$' respectively. Such a pair of sentences will be called a reduction pair for 'Q_3'. By R_1 the property Q_3 is attributed to the points of the class $Q_1 \cdot Q_2$, by R_2 the property $\sim Q_3$ to the points of the class $Q_4 \cdot Q_5$. If by the rules of the language – either logical rules or physical laws – we can show that no point belongs to either of these classes (in other words, if the universal sentence '$\sim [(Q_1 \cdot Q_2) \vee (Q_4 \cdot Q_5)]$' is valid) then the pair of sentences does not determine Q_3 nor $\sim Q_3$ for any point and therefore does not give a reduction for the predicate Q_3. Therefore, in the definition of 'reduction pair' to be stated, we must exclude this case.

In special cases 'Q_4' coincides with 'Q_1', and 'Q_5' with '$\sim Q_2$'.

In that case the reduction pair is '$Q_1 \supset (Q_2 \supset Q_3)$' and '$Q_1 \supset (\sim Q_2 \supset \sim Q_3)$'; the latter can be transformed into '$Q_1 \supset (Q_3 \supset Q_2)$'. Here the pair can be replaced by the one sentence '$Q_1 \supset (Q_3 \equiv Q_2)$' which means: if we accomplish the condition Q_1, then the point has the property Q_3 if and only if we find the result Q_2. This sentence may serve for determining the result '$Q_3(b)$' as well as for '$\sim Q_3(b)$'; we shall call it a bilateral reduction sentence. It determines Q_3 for the points of the class $Q_1 \cdot Q_2$, and $\sim Q_3$ for those of the class $Q_1 \cdot \sim Q_2$; it does not give a determination for the points of the class $\sim Q_1$. Therefore, if '$(x)(\sim Q_1(x))$' is valid, the sentence does not give any determination at all. To give an example, let '$Q'_1(b)$' mean "the point b is both heated and not heated", and '$Q''_1(b)$': "the point b is illuminated by light-rays which have a speed of 400,000 km/sec". Here for any point c, '$Q'_1(c)$' and '$Q''_1(c)$' are contravalid – the first contradictory and the second P-contravalid; therefore, '$(x)(\sim Q_1'(x))$' and '$(x)(\sim Q_1''(x))$' are valid – the first analytic and the second P-valid; in other words, the conditions Q'_1 and Q''_1 are impossible, the first logically and the second physically. In this case, a sentence of the form '$Q'_1 \supset (Q_3 \equiv Q_2)$' or '$Q''_1 \supset (Q_3 \equiv Q_2)$' would not tell us anything about how to use the predicate 'Q_3' and therefore could not be taken as a reduction sentence. These considerations lead to the following definitions.

Definition 10. a. A universal sentence of the form

(R) $$Q_1 \supset (Q_2 \supset Q_3)$$

is called a *reduction sentence* for 'Q_3' provided '$\sim (Q_1 \cdot Q_2)$' is not valid.

b. A pair of sentences of the forms

(R_1) $$Q_1 \supset (Q_2 \supset Q_3)$$
(R_2) $$Q_4 \supset (Q_5 \supset \sim Q_3)$$

is called a *reduction pair* for 'Q_3' provided '$\sim [(Q_1 \cdot Q_2) \vee (Q_4 \cdot Q_5)]$' is not valid.

c. A sentence of the form

(R_b) $$Q_1 \supset (Q_3 \equiv Q_2)$$

is called a *bilateral reduction sentence* for 'Q_3' provided '$(x)(\sim Q_1(x))$' is not valid.

Every statement about reduction pairs in what follows applies also to bilateral reduction sentences, because such sentences are comprehensive formulations of a special case of a reduction pair.

If a reduction pair for 'Q_3' of the form given above is valid – i.e. either laid down in order to introduce 'Q_3' on the basis of 'Q_1', 'Q_2', 'Q_4', and 'Q_5', or consequences of physical laws stated beforehand – then for any point c '$Q_3(c)$' is a consequence of '$Q_1(c)$' and '$Q_2(c)$', and '$\sim Q_3(c)$' is a consequence of '$Q_4(c)$' and '$Q_5(c)$'. Hence 'Q_3' is completely reducible to those four predicates.

Theorem 6. If a reduction pair for 'Q' is valid, then 'Q' is completely reducible to the four (or two, respectively) other predicates occurring.

We may distinguish between logical reduction and physical reduction, dependent upon the reduction sentence being analytic or P-valid, in the latter case for instance a valid physical law. Sometimes not only the sentence '$Q_1 \supset (Q_3 \equiv Q_2)$' is valid, but also the sentence '$Q_3 \equiv Q_2$'. (This is e.g. the case if '$(x)Q_1(x)$' is valid.) Then for any b, '$Q_3(b)$' can be transformed into the equipollent sentence '$Q_2(b)$', and thus 'Q_3' can be eliminated in any sentence whatever. If '$Q_3 \equiv Q_2$' is not P-valid but analytic it may be considered as an explicit definition for 'Q_3'. Thus an *explicit definition* is a special kind of a logical bilateral reduction sentence. A logical bilateral reduction sentence which does not have this simple form, but the general form '$Q_1 \supset (Q_3 \equiv Q_2)$', may be considered as a kind of conditioned definition.

If we wish to construct a language for science we have to take some descriptive (i.e. non-logical) terms as primitive terms. Further terms may then be introduced not only by explicit definitions but also by other reduction sentences. The possibility of *introduction by laws*, i.e. by physical reduction, is, as we shall see, very important for science, but so far not sufficiently noticed in the logical analysis of science. On the other hand the terms introduced in this way have the disadvantage that in general it is not possible to eliminate them, i.e. to translate a sentence containing such a term into a sentence containing previous terms only.

Let us suppose that the term 'Q_3' does not occur so far in our language, but 'Q_1', 'Q_2', 'Q_4', and 'Q_5' do occur. Suppose further that either the following reduction pair R_1, R_2 for 'Q_3':

$$(R_1) \quad Q_1 \supset (Q_2 \supset Q_3)$$
$$(R_2) \quad Q_4 \supset (Q_5 \supset \sim Q_3)$$

or the following bilateral reduction sentence for 'Q_3':

$$(R_b) \quad Q_1 \supset (Q_3 \equiv Q_2)$$

is stated as valid in order to introduce 'Q_3', i.e. to give meaning to this new term of our language. Since, on the assumption made, 'Q_3' has no antecedent meaning, we do not assert anything about facts by the statement of R_b. This statement is not an assertion but a convention. In other words, the factual content of R_b is empty; in this respect, R_b is similar to a definition. On the other hand, the pair R_1, R_2 has a positive content. By stating it as valid, beside stating a convention concerning the use of the term 'Q_3', we assert something about facts that can be formulated in the following way without the use of 'Q_3'. If a point c had the property $Q_1 \cdot Q_2 \cdot Q_4 \cdot Q_5$, then both '$Q_3(c)$' and '$\sim Q_3(c)$' would follow. Since this is not possible for any point, the following universal sentence S which does not contain 'Q_3', and which in general is synthetic, is a consequence of R_1 and R_2:

$$(S:) \quad \sim (Q_1 \cdot Q_2 \cdot Q_4 \cdot Q_5).$$

In the case of the bilateral reduction sentence R_b 'Q_4' coincides with 'Q_1' and 'Q_5' with '$\sim Q_2$'. Therefore in this case S degenerates to '$\sim (Q_1 \cdot Q_2 \cdot Q_1 \cdot \sim Q_2)$' and hence becomes analytic. Thus a bilateral reduction sentence, in contrast to a reduction pair, has no factual content.

9. Introductive Chains

For the sake of simplicity we have considered so far only the introduction of a predicate by one reduction pair or by one bilateral reduction sentence. But in most cases a predicate will be introduced by either several reduction pairs or several bilateral reduction sentences. If a property or physical magnitude can

be determined by different methods then we may state one reduction pair or one bilateral reduction sentence for each method. The intensity of an electric current can be measured for instance by measuring the heat produced in the conductor, or the deviation of a magnetic needle, or the quantity of silver separated out of a solution, or the quantity of hydrogen separated out of water etc. We may state a set of bilateral reduction sentences, one corresponding to each of these methods. The factual content of this set is not null because it comprehends such sentences as e.g. "If the deviation of a magnetic needle is such and such then the quantity of silver separated in one minute is such and such, and vice versa" which do not contain the term 'intensity of electric current', and which obviously are synthetic.

If we establish one reduction pair (or one bilateral reduction sentence) as valid in order to introduce a predicate 'Q_3', the meaning of 'Q_3' is not established completely, but only for the cases in which the test condition is fulfilled. In other cases, e.g. for the match in our previous example, neither the predicate nor its negation can be attributed. We may diminish this region of indeterminateness of the predicate by adding one or several more laws which contain the predicate and connect it with other terms available in our language. These further laws may have the form of reduction sentences (as in the example of the electric current) or a different form. In the case of the predicate 'soluble in water' we may perhaps add the law stating that two bodies of the same substance are either both soluble or both not soluble. This law would help in the instance of the match; it would, in accordance with common usage, lead to the result "the match c is not soluble," because other pieces of wood are found to be insoluble on the basis of the first reduction sentence. Nevertheless, a region of indeterminateness remains, though a smaller one. If a body b consists of such a substance that for no body of this substance has the test-condition – in the above example: "being placed into water" – ever been fulfilled, then neither the predicate nor its negation can be attributed to b. This region may then be diminished still further, step by step, by stating new laws. These laws do not have the conventional character that definitions have;

rather are they discovered empirically within the region of meaning which the predicate in question received by the laws stated before. But these laws are extended by convention into a region in which the predicate had no meaning previously; in other words, we decided to use the predicate in such a way that these laws which are tested and confirmed in cases in which the predicate has a meaning, remain valid in other cases.

We have seen that a new predicate need not be introduced by a definition, but may equally well be introduced by a set of reduction pairs. (A bilateral reduction sentence may here be taken as a special form of a reduction pair.) Consequently, instead of the usual chain of definitions, we obtain a chain of sets of sentences, each set consisting either of one definition or of one or several reduction pairs. By each set a new predicate is introduced.

Definition 11. A (finite) chain of (finite) sets of sentences is called an *introductive chain* based upon the class C of predicates if the following conditions are fulfilled. Each set of the chain consists either of one definition or of one or more reduction pairs for one predicate, say 'Q'; every predicate occurring in the set, other than 'Q', either belongs to C or is such that one of the previous sets of the chain is either a definition for it or a set of reduction pairs for it.

Definition 12. If the last set of a given introductive chain based upon C either consists in a definition for 'Q' or in a set of reduction pairs for 'Q', 'Q' is said to be *introduced* by this chain on the basis of C.

For our purposes we will suppose that a reduction sentence always has the simple form '$Q_1 \supset (Q_2 \supset Q_3)$' and not the analogous but more complicated form '$(x)\,[---x--- \supset (\ldots x \ldots \supset Q_3(x))]$' where '$---x---$' and '$\ldots x \ldots$' indicate sentential functions of a non-atomic form. This supposition does not restrict the generality of the following considerations because a reduction sentence of the compound form indicated may always be replaced by two definitions and a reduction sentence of the simple form, namely by:

$$Q_1 \equiv ---x---$$
$$Q_2 \equiv \cdots x \cdots$$
$$Q_1 \supset (Q_2 \supset Q_3).$$

The above supposition once made, the nature of an introductive chain is chiefly dependent upon the form of the definitions occurring. Therefore we define as follows.

Definition 13. An introductive chain is said to have atomic form (or molecular form) if every definition occurring in it has atomic form (or molecular form, respectively); it is said to have generalized form (or essentially generalized form) if at least one definition of generalized form (or essentially generalized form, respectively) occurs in it.

Theorem 7. If 'P' is introduced by an introductive chain based upon C, 'P' is reducible to C. If the chain has molecular form, 'P' is completely reducible to C; if the chain has essentially generalized form, 'P' is incompletely reducible to C. – This follows from Theorems 5 (§ 7) and 6 (§ 8).

We call *primitive symbols* those symbols of a language L which are introduced directly, i.e. without the help of other symbols. Thus there are the following kinds of symbols of L:

1) *primitive symbols* of L,
2) *indirectly introduced symbols*, i.e. those introduced by introductive chains based upon primitive symbols; here we distinguish:
   a) *defined symbols*, introduced by chains of definitions,
   b) *reduced symbols*, i.e. those introduced by introductive chains containing at least one reduction sentence; here we may further distinguish:
      α) *L-reduced symbols*, whose chains contain only L-reduction pairs,
      β) *P-reduced symbols*, whose chains contain at least one P-reduction pair.

Definition 14. a. An *introductive chain* based upon primitive predicates of a language L and having atomic (or molecular, or generalized, or essentially generalized, respectively) form is called an atomic (or molecular, or generalized, or essentially generalized, respectively) introductive chain of L.

b. A *predicate* of L is called an *atomic* (or *molecular*) predicate if it is either a primitive predicate of L or introduced by an atomic (or molecular, respectively) introductive chain of L; it is called a *generalized* (or essentially generalized) predicate if it is intro-

duced by a generalized (or essentially generalized, respectively) introductive chain of L.

Definition 15. a. A sentence S is called an *atomic sentence* if S is a full sentence of an atomic predicate. – b. S is called a *molecular sentence* if S has molecular form and contains only molecular predicates. – c. S is called a *generalized sentence* if S contains an (unrestricted) operator or a generalized predicate. – d. S is called an essentially generalized sentence if S is a generalized sentence and is not equipollent with a molecular sentence.

It should be noticed that the term 'atomic sentence', as here defined, is not at all understood to refer to ultimate facts.[28] Our theory does not assume anything like ultimate facts. It is a matter of convention which predicates are taken as primitive predicates of a certain language L; and hence likewise, which predicates are taken as atomic predicates and which sentences as atomic sentences.

10. Reduction and Definition

In § 8 the fact was mentioned that in some cases, for instance in the case of a disposition-term, the reduction cannot be replaced by a definition. We now are in a position to see the situation more clearly. Suppose that we introduce a predicate 'Q' into the language of science first by a reduction pair and that, later on, step by step, we add more such pairs for 'Q' as our knowledge about 'Q' increases with further experimental investigations. In the course of this procedure the range of indeterminateness for 'Q', i.e. the class of cases for which we have not yet given a meaning to 'Q', becomes smaller and smaller. Now at each stage of this development we could lay down a definition for 'Q' corresponding to the set of reduction pairs for 'Q' established up to that stage. But, in stating the definition, we should have to make an arbitrary decision concerning the cases which are not determined by the set of reduction pairs. A definition determines the meaning of the new term once for all. We could either decide to attribute 'Q' in the cases not determined by the set, or to

[28] In contradistinction to the term 'atomic sentence' or 'elementary sentence' as used by Russell or Wittgenstein.

attribute '$\sim Q$' in these cases. Thus for instance, if a bilateral reduction sentence R of the form '$Q_1 \supset (Q_3 \equiv Q_2)$' is stated for '$Q_3$', then the predicate '$Q_3$' is to be attributed to the points of the class $Q_1 \cdot Q_2$, and '$\sim Q_3$' to those of the class $Q_1 \cdot \sim Q_2$, while for the points of the class $\sim Q_1$ the predicate 'Q_3' has no meaning. Now we might state one of the following two definitions:

$$(D_1) \qquad Q_3 \equiv (Q_1 \cdot Q_2)$$
$$(D_2) \qquad Q_3 \equiv (\sim Q_1 \vee Q_2)$$

If c is a point of the undetermined class, on the basis of D_1 '$Q_3(c)$' is false, and on the basis of D_2 it is true. Although it is possible to lay down either D_1 or D_2, neither procedure is in accordance with the intention of the scientist concerning the use of the predicate 'Q_3'. The scientist wishes neither to determine all the cases of the third class positively, nor all of them negatively; he wishes to leave these questions open until the results of further investigations suggest the statement of a new reduction pair; thereby some of the cases so far undetermined become determined positively and some negatively. If we now were to state a definition, we should have to revoke it at such a new stage of the development of science, and to state a new definition, incompatible with the first one. If, on the other hand, we were now to state a reduction pair, we should merely have to add one or more reduction pairs at the new stage; and these pairs will be compatible with the first one. In this latter case we do not correct the determinations laid down in the previous stage but simply supplement them.

Thus, if we wish to introduce a new term into the language of science, we have to distinguish two cases. If the situation is such that we wish to fix the meaning of the new term once for all, then a definition is the appropriate form. On the other hand, if we wish to determine the meaning of the term at the present time for some cases only, leaving its further determination for other cases to decisions which we intend to make step by step, on the basis of empirical knowledge which we expect to obtain in the future, then the method of reduction is the appropriate one rather than that of a definition. A set of reduction pairs is a partial determination of meaning only and can therefore not be replaced by a

definition. Only if we reach, by adding more and more reduction pairs, a stage in which all cases are determined, may we go over to the form of a definition.

We will examine in greater detail the situation in the case of several reduction pairs for 'Q_3':

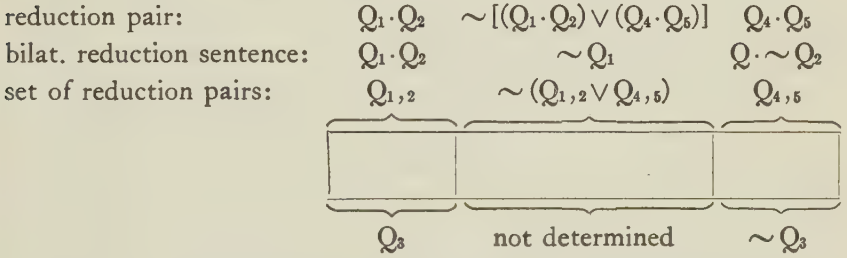
(R_1) $\quad Q_1 \supset (Q_2 \supset Q_3)$
(R_2) $\quad Q_4 \supset (Q_5 \supset \sim Q_3)$
(R_1') $\quad Q_1' \supset (Q_2' \supset Q_3)$
(R_2') $\quad Q_4' \supset (Q_5' \supset \sim Q_3)$
etc.

Then 'Q_3' is determined by R_1 for the points of the class $Q_1 \cdot Q_2$, by R_1' for the class $Q_1' \cdot Q_2'$, etc., and therefore, by the totality of reduction sentences for 'Q_3', for the class $(Q_1 \cdot Q_2) \vee (Q_1' \cdot Q_2') \vee \ldots$. This class may shortly be designated by '$Q_{1,2}$'. Analogously '$\sim Q_3$' is determined by the reduction sentences for '$\sim Q_3$' for the points of the class $(Q_4 \cdot Q_5) \vee (Q_4' \cdot Q_5') \vee \ldots$, which we designate by '$Q_{4,5}$'. Hence 'Q_3' is determined either positively or negatively for the class $Q_{1,2} \vee Q_{4,5}$. Therefore the universal sentence '$Q_{1,2} \vee Q_{4,5}$' means, that for every point either 'Q_3' or '$\sim Q_3$' is determined. If this sentence is true, the set of reduction sentences is complete and may be replaced by the definition '$Q_3 \equiv Q_{1,2}$'. For the points of the class $\sim (Q_{1,2} \vee Q_{4,5})$, '$Q_3$' is not determined, and hence, in the stage in question, 'Q_3' is without meaning for these points. If on the basis of either logical rules or physical laws it can be shown that all points belong to this class, in other words, if the universal sentence '$\sim (Q_{1,2} \vee Q_{4,5})$' is valid – either analytic or P-valid – then neither 'Q_3' nor '$\sim Q_3$' is determined for any point and hence the given set of reduction pairs does not even partly determine the meaning of 'Q_3' and therefore is not a suitable means of introducing this predicate.

The given set of reduction pairs asserts that a point belonging to the class $Q_{4,5}$ has the property $\sim Q_3$ and hence not the property Q_3, and therefore cannot belong to $Q_{1,2}$ because every point of this class has the property Q_3. What the set asserts can therefore be formulated by the universal sentence saying that no point belongs

to both $Q_{1,2}$ and $Q_{4,5}$, i.e. the sentence '$\sim (Q_{1,2} \cdot Q_{4,5})$'. This sentence represents, so to speak, the factual content of the set. In the case of one reduction pair this representative sentence is '$\sim (Q_1 \cdot Q_2 \cdot Q_4 \cdot Q_5)$'; in the case of one bilateral reduction sentence this becomes '$\sim (Q_1 \cdot Q_2 \cdot Q_1 \cdot \sim Q_2)$' or '$(x)(Q_2(x) \vee \sim Q_2(x))$', which is analytic.

The following diagram shows the tripartition of the class of all points by a reduction pair (or a bilateral reduction sentence, or a set of reduction pairs, respectively). For the first class 'Q_3' is determined, for the second class '$\sim Q_3$'. The third class lies between them and is not yet determined; but some of its points may be determined as belonging to Q_3 and some others as belonging to $\sim Q_3$ by reduction pairs to be stated in the future.



If we establish a set of *reduction pairs* as new valid sentences for the introduction of a new predicate 'Q'_3', are these valid sentences *analytic or P-valid*? Moreover, which other sentences containing 'Q_3' are analytic? The distinction between analytic and P-valid sentences refers primarily to those sentences only in which all descriptive terms are primitive terms. In this case the criterion is as follows:[29] a valid sentence S is analytic if and only if every sentence S′ is also valid which is obtained from S when any descriptive term wherever it occurs in S is replaced by any other term whatever of the same type; otherwise it is P-valid. A sentence S containing defined terms is analytic if the sentence S′ resulting from S by the elimination of the defined terms is analytic; otherwise it is P-valid. A definition, e.g. '$Q(x) \equiv \ldots x \ldots$'

[29] Carnap [4] §51.

is, according to this criterion, itself analytic; for, after it has been stated as a valid sentence, by the elimination of 'Q' we get from it '. . . x . . . ≡ . . . x . . .', which is analytic.

In the case of a new descriptive term introduced by a set of reduction pairs, the situation is not as simple as in the case of a definition because elimination is here not possible. Let us consider the question how the criterion is to be stated in this case. The introduction of a new term into a language is, strictly speaking, the construction of a new language on the basis of the original one. Suppose that we go over from the language L_1, which does not contain 'Q', to the language L_2 by introducing 'Q' by a set R of reduction pairs, whose representative sentence (in the sense explained before) may be taken to be S. Then S as not containing 'Q' is a sentence of L_1 also; its logical character within L_1 does not depend upon 'Q' and may therefore be supposed to be determined already. By stating the sentences of R as valid in L_2, S becomes also valid in L_2 because it is a consequence of R in L_2. If now S is analytic in L_1, it is also analytic in L_2; in this case R does not assert anything about facts, and we must therefore take its sentences as analytic. According to this, every bilateral reduction sentence is analytic, because its representative sentence is analytic, as we have seen before. If S is either P-valid or indeterminate in L_1, it is valid and moreover P-valid in L_2 in consequence of our stating R as valid in L_2. In this case every sentence of R is valid; it is P-valid unless it fulfills the general criterion of analyticity stated before (referring to all possible replacements of the descriptive terms, see p. 451). If S is either P-contravalid or contradictory in L_1, it has the same property in L_2 and is simultaneously valid in L_2. It may be analytic in L_2, if it fulfills the general criterion. In this case every sentence of R is both valid and contravalid, and hence L_2 is inconsistent.[30] If S is contradictory in L_1 and at least one sentence of R is analytic according to the general criterion, then L_2 is not only inconsistent but also L-inconsistent. The results of these considerations may be exhibited by the following table; column (1) gives a complete classification of the sentences of a language (see the diagram in § 5).

[30] Compare Carnap [4] §59.

The representative sentence S		a reduction sentence of R (in L_2)	L_2
in L_1	in L_2		
1. analytic	analytic	analytic	consistent (if L_1 is consistent)
2. P-valid	P-valid	valid*	
3. indeterminate	P-valid	valid*	
4. P-contravalid	valid and P-contravalid	valid* and P-contravalid	inconsistent
5. contradictory	valid and contradictory	valid* and contradictory	inconsistent†

* analytic if fulfilling the general criterion (p. 451); otherwise P-valid.

† and moreover L-inconsistent if at least one sentence of R is analytic on the basis of the general criterion (p. 451).

Now the *complete criterion for 'analytic'* can be stated as follows:

Nature of S	Criterion for S being *analytic*
1. S does not contain any descriptive symbol.	S is valid.
2. All descriptive symbols of S are primitive.	Every sentence S′ which results from S when we replace any descriptive symbol at all places where it occurs in S by any symbol whatever of the same type—and hence S itself also—is valid.
3. S contains a defined descriptive symbol 'Q'.	The sentence S′ resulting from S by the elimination of 'Q' is valid.
4. S contains a descriptive symbol 'Q' introduced by a set R of reduction pairs; let L′ be the sublanguage of L not containing 'Q', and S′ the representative sentence of R (comp. p. 451).	S′ is analytic in L′, and S is an L-consequence of R (e.g. one of the sentences of R); in other words, the implication sentence containing the conjunction of the sentences of R as first part and S as second part is analytic (i.e. every sentence resulting from this implication sentence where we replace 'Q' at all places by any symbol of the same type occurring in L′ is valid in L′).

III. EMPIRICAL ANALYSIS OF CONFIRMATION AND TESTING

11. Observable and Realizable Predicates

In the preceding chapter we analyzed logically the relations which subsist among sentences or among predicates if one of them may be confirmed with the help of others. We defined some concepts of a syntactical kind, based upon the concept 'consequence' as the chief concept of logical syntax. In what follows we shall deal with *empirical methodology*. Here also we are concerned with the questions of confirming and testing sentences and predicates. These considerations belong to a theory of language just as the logical ones do. But while the logical analysis belongs to an analytic theory of the formal, syntactical structure of language, here we will carry out an empirical analysis of the application of language. Our considerations belong, strictly speaking, to a biological or psychological theory of language as a kind of human behavior, and especially as a kind of reaction to observations. We shall see, however, that for our purposes we need not go into details of biological or psychological investigations. In order to make clear what is understood by empirically testing and confirming a sentence and thereby to find out what is to be required for a sentence or a predicate in a language having empirical meaning, we can restrict ourselves to using very few concepts of the field mentioned. We shall take two descriptive, i.e. non-logical, terms of this field as *basic terms* for our following considerations, namely '*observable*' and '*realizable*'. All other terms, and above all the terms 'confirmable' and 'testable', which are the chief terms of our theory, will be defined on the basis of the two basic terms mentioned; in the definitions we shall make use of the logical terms defined in the foregoing chapter. The two basic terms are of course, as basic ones, not defined within our theory. Definitions for them would have to be given within psychology, and more precisely, within the behavioristic theory of language. We do not attempt such definitions, but we shall give at least some rough explanations for the terms, which will make their meaning clear enough for our purposes.

Explanation 1. A predicate 'P' of a language L is called *observable* for an organism (e.g. a person) N, if, for suitable argu-

ments, e.g. 'b', N is able under suitable circumstances to come to a decision with the help of few observations about a full sentence, say 'P(b)', i.e. to a confirmation of either 'P(b)' or '$\sim$P(b)' of such a high degree that he will either accept or reject 'P(b)'.

This explanation is necessarily vague. There is no sharp line between observable and non-observable predicates because a person will be more or less able to decide a certain sentence quickly, i.e. he will be inclined after a certain period of observation to accept the sentence. For the sake of simplicity we will here draw a sharp distinction between observable and non-observable predicates. By thus drawing an arbitrary line between observable and non-observable predicates in a field of continuous degrees of observability we partly determine in advance the possible answers to questions such as whether or not a certain predicate is observable by a given person. Nevertheless the general philosophical, i.e. methodological question about the nature of meaning and testability will, as we shall see, not be distorted by our oversimplification. Even particular questions as to whether or not a given sentence is confirmable, and whether or not it is testable by a certain person, are affected, as we shall see, at most to a very small degree by the choice of the boundary line for observable predicates.

According to the explanation given, for example the predicate 'red' is observable for a person N possessing a normal colour sense. For a suitable argument, namely a space-time-point c sufficiently near to N, say a spot on the table before N, N is able under suitable circumstances – namely, if there is sufficient light at c – to come to a decision about the full sentence "the spot c is red" after few observations – namely by looking at the table. On the other hand, the predicate 'red' is not observable by a colour-blind person. And the predicate 'an electric field of such and such an amount' is not observable to anybody, because, although we know how to test a full sentence of this predicate, we cannot do it directly, i.e. by a few observations; we have to apply certain instruments and hence to make a great many preliminary observations in order to find out whether the things before us are instruments of the kind required.

Explanation 2. A predicate 'P' of a language L is called

'*realizable*' by N, if for a suitable argument, e.g. 'b', N is able under suitable circumstances to make the full sentence 'P(b)' true, i.e. to produce the property P at the point b.

When we use the terms 'observable' and 'realizable' without explicit reference to anybody, it is to be understood that they are meant with respect to the people who use the language L to which the predicate in question belongs.

Examples. Let 'P_1(b)' mean: 'the space-time-point b has the temperature 100°C'. 'P_1' is realizable by us because we know how to produce that temperature at the point b, if b is accessible to us. – 'P_2(b)' may mean: 'there is iron at the point b'. 'P_2' is realizable because we are able to carry a piece of iron to the point b if b is accessible. – If 'P_3(b)' means: 'at the point b is a substance whose index of light refraction is 10', 'P_3' is not realizable by anybody at the present time, because nobody knows at present how to produce such a substance.

12. Confirmability

In the preceding chapter we have dealt with the concept of reducibility of a predicate 'P' to a class C of other predicates, i.e. the logical relation which subsists between 'P' and C if the confirmation of 'P' can be carried out by that of predicates of C. Now, if confirmation is to be feasible at all, this process of referring back to other predicates must terminate at some point. The reduction must finally come to predicates for which we can come to a confirmation directly, i.e. without reference to other predicates. According to Explanation 1, the observable predicates can be used as such a basis. This consideration leads us to the following definition of the concept 'confirmable'. This concept is a descriptive one, in contradistinction to the logical concept 'reducible to C' – which could be named also 'confirmable with respect to C'.

Definition 16. A *sentence* S is called *confirmable* (or completely confirmable, or incompletely confirmable) if the confirmation of S is reducible (or completely reducible, or incompletely reducible, respectively) to that of a class of observable predicates.

Definition 17. A sentence S is called *bilaterally confirmable*

(or bilaterally completely confirmable) if both S and $\sim$S are confirmable (or completely confirmable, respectively).

Definition 18. A *predicate* 'P' is called *confirmable* (or completely confirmable, or incompletely confirmable) if 'P' is reducible (or completely reducible, or incompletely reducible, respectively) to a class of observable predicates.

Hence, if 'P' is confirmable (or completely confirmable) the full sentences of 'P' are bilaterally confirmable (or bilaterally completely confirmable, respectively).

When we call a sentence S confirmable, we do not mean that it is possible to arrive at a confirmation of S under the circumstances as they actually exist. We rather intend this possibility under some *possible circumstances*, whether they be real or not. Thus e.g. because my pencil is black and I am able to make out by visual observation that it is black and not red, I cannot come to a positive confirmation of the sentence "My pencil is red." Nevertheless we call this sentence confirmable and moreover completely confirmable for the reason that we are able to indicate the – actually non-existent, but possible – observations which would confirm that sentence. Whether the real circumstances are such that the testing of a certain sentence S leads to a positive result, i.e. to a confirmation of S, or such that it leads to a negative result, i.e. to a confirmation of $\sim$S, is irrelevant for the questions of confirmability, testability and meaning of the sentence though decisive for the question of truth i.e. sufficient confirmation.

Theorem 8. If 'P' is introduced on the basis of observable predicates, 'P' is confirmable. If the introductive chain has molecular form, 'P' is completely confirmable. – This follows from Theorem 7 (§ 9).

Theorem 9. If S is a sentence of molecular form and all predicates occurring in S are confirmable (or completely confirmable) S is bilaterally confirmable (or bilaterally completely confirmable, respectively). – From Theorem 2 (§ 6).

Theorem 10. If the sentence S is constructed out of confirmable predicates with the help of connective symbols and universal or existential operators, S is bilaterally confirmable. – From Theorems 2, 3, and 4 (§ 6).

13. Method of Testing

If 'P' is confirmable then it is not impossible that for a suitable point *b* we may find a confirmation of 'P(b)' or of '$\sim$ P(b)'. But it is not necessary that we know a method for finding such a confirmation. If such a procedure can be given – we may call it a *method of testing* – then 'P' is not only confirmable but – as we shall say later on – testable. The following considerations will deal with the question how to formulate a method of testing and thereby will lead to a definition of 'testable'.

The description of a method of testing for 'Q_3' has to contain two other predicates of the following kinds:

1) A predicate, say 'Q_1', describing a *test-condition* for 'Q_3', i.e. an experimental situation which we have to create in order to test 'Q_3' at a given point.

2) A predicate, say 'Q_2', describing a *truth-condition* for 'Q_3' with respect to 'Q_1', i.e. a possible experimental result of the test-condition Q_1 at a given point *b* of such a kind that, if this result occurs, 'Q_3' is to be attributed to *b*. Now the connection between 'Q_1', 'Q_2', and 'Q_3' is obviously as follows: if the test-condition is realized at the given point *b* then, if the truth-condition is found to be fulfilled at *b*, *b* has the property to be tested; and this holds for any point. Thus the method of testing for 'Q_3' is to be formulated by the universal sentence '$Q_1 \supset (Q_2 \supset Q_3)$', in other words, by a reduction sentence for 'Q_3'. But this sentence, beside being a reduction sentence, must fulfill the following two additional requirements:

1) 'Q_1' must be realizable because, if we did not know how to produce the test-condition, we could not say that we had a method of testing.

2) We must know beforehand how to test the truth condition Q_2; otherwise we could not test 'Q_3' although it might be confirmable. In order to satisfy the second requirement, 'Q_2' must be either observable or explicitly defined on the basis of observable predicates or a method of testing for it must have been stated. If we start from observable predicates – which, as we know, can be tested without a description of a method of testing being necessary – and then introduce other predicates by explicit

definitions or by such reduction sentences as fulfill the requirements stated above and hence are descriptions of a method of testing, then we know how to test each of these predicates. Thus we are led to the following definitions.

Definition 19. An introductive chain of such a kind that in each of its reduction sentences, say '$Q_1 \supset (Q_2 \supset Q_3)$' or '$Q_4 \supset (Q_5 \supset \sim Q_3)$', the first predicate – 'Q_1' or 'Q_4', respectively – is realizable, is called a *test chain*. A reduction sentence (or a reduction pair, or a bilateral reduction sentence) belonging to a test chain is called a *test sentence* (or a *test pair*, or a *bilateral test sentence*, respectively).

A test pair for 'Q', and likewise a bilateral test sentence for 'Q', describes a method of testing for both 'Q' and '$\sim$ Q'. A bilateral test sentence, e.g. '$Q_1 \supset (Q_3 \equiv Q_2)$' may be interpreted in words in the following way: "If at a space-time-point x the test-condition Q_1 (consisting perhaps in a certain experimental situation, including suitable measuring instruments) is realized then we will attribute the predicate 'Q_3' to the point *x* if and only if we find at *x* the state Q_2 (which may be a certain result of the experiment, e.g. a certain position of the pointer on the scale)". To give an example, let 'Q_3(b)' mean: "The fluid at the space-time-point *b* has a temperature of 100°"; 'Q_1(b)': "A mercury thermometer is put at *b*; we wait, while stirring the liquid, until the mercury comes to a standstill"; 'Q_2(b)': "The head of the mercury column of the thermometer at *b* stands at the mark 100 of the scale." If here 'Q_3' is introduced by '$Q_1 \supset (Q_3 \equiv Q_2)$' obviously its testability is assured.

14. Testability

Definition 20. If a predicate is either observable or introduced by a test chain it is called *testable*. A testable predicate is called *completely testable* if it is either observable or introduced by a test chain having molecular form; otherwise *incompletely testable*.

Let us consider the question under what conditions a set of laws, e.g. of physics, which contain a predicate 'Q' can be transformed into a set of reduction-sentences or of test-sentences for 'Q'. Suppose a set of laws is given which contain 'Q' and have

the following form. Each of the laws is a universal sentence containing only individual variables (no predicate variables); 'Q' is followed wherever it occurs in the sentence by the same set of variables, which are bound by universal operators applying to the whole sentence. Thus each of the laws has the form '(x)[. . . Q(x) . . . Q(x) . . .]'. The majority of the laws of classical physics can be brought into this form. Now the given set of laws can be transformed in the following way. First we write down the conjunction of the laws of the given set and transform it into one universal sentence '(x)[. . . Q(x) . . . Q(x) . . .]'. Then we transform the function included in square brackets into the so-called conjunctive normal form,[31] i.e. a conjunction of say n disjunctions of such a kind that 'Q' occurs only in partial sentences which are members of such disjunctions and have either the form 'Q(x)' or '$\sim$ Q(x)'. Finally we dissolve the whole universal sentence into n universal sentences in accordance with the rule that '$(x)[P_1(x) \cdot P_2(x) \cdot \ldots \cdot P_n(x)]$' can be transformed into '$(x)P_1(x) \cdot (x)P_2(x) \cdot \ldots (x)P_n(x)$'. Thus we have a set of n universal sentences; each of them is a disjunction having among its members either 'Q(x)' or '$\sim$Q(x)' or both. If we employ '$\sim$ P(x)' as abbreviation for the disjunction of the remaining members not containing 'Q' these sentences have one of the following forms:

1. $\quad Q \vee \sim P,$
2. $\quad \sim Q \vee \sim P$
3. $\quad Q \vee \sim Q \vee \sim P.$

A sentence of the form (3) is analytic and can therefore be omitted without changing the content of the set. (1) can be given the form '$P \supset Q$' and, by analysing 'P' in some way or other into a conjunction '$P_1 \cdot P_2$', the form '$(P_1 \cdot P_2) \supset Q$' and hence '$P_1 \supset (P_2 \supset Q)$' which is a reduction sentence of the first form. In the same way (2) can be transformed into '$P \supset \sim Q$' and hence into '$(P_1 \cdot P_2) \supset \sim Q$' and into '$P_1 \supset (P_2 \supset \sim Q)$' which is a reduction sentence of the second form. An analysis of 'P' into '$P_1 \cdot P_2$' is obviously always possible; if not otherwise then in the triv-

[31] Compare Hilbert [1] p. 13; Carnap [4b] §34b, RR 2.

ial way of taking an analytic predicate as 'P_1' and 'P' itself as 'P_2'. If 'P' is testable then we may look for such an analysis that 'P_1' is realizable. If we can find such a one then – since 'P_2' is also testable in this case – the reduction sentence '$P_1 \supset (P_2 \supset Q)$' or '$P_1 \supset (P_2 \supset \sim Q)$' is a test-sentence.

Thus we have seen that a set of laws of the form here supposed can always be transformed into a set of reduction sentences for 'Q', and, if a special condition is fulfilled, into a set of test-sentences. This condition is fulfilled in very many and perhaps most of the cases actually occurring in physics because nearly all predicates used in physics are testable and perhaps most of them are realizable. –

Theorem 11. If a predicate is testable it is confirmable; if it is completely testable it is completely confirmable. – By Theorem 8, §12.

On the other hand, 'P' may be *confirmable without being testable.* This is the case, if 'P' is introduced by an introductive chain based upon observable predicates but containing a reduction sentence '$Q_1 \supset (Q_2 \supset Q_3)$' of such a kind that '$Q_1$', although it is of course confirmable and may even be testable, is not realizable. If this should be the case, there is a possibility that by a happy chance the property Q_3 will be found at a certain point, although we have no method which would lead us with certainty to such a result. Suppose that 'Q_1' and 'Q_2' are completely confirmable, i.e. completely reducible to observable predicates – they may even be observable themselves – and that 'Q_3' is introduced by '$Q_1 \supset (Q_3 \equiv Q_2)$'. Let c be a point in our spatio-temporal neighborhood such that we are able to observe its properties. Then by happy chance '$Q_1(c)$' may be true. If so, we are able to find this out by observation and then, by either finding '$Q_2(c)$' or '$\sim Q_2(c)$', to arrive at the conclusion either of '$Q_3(c)$' or of '$\sim Q_3(c)$'. But if that stroke of luck does not happen, i.e. if '$Q_1(c)$' is false – no matter whether we find that out by our observations or not – we are not in a position to determine the truth or falsehood of '$Q_3(c)$', and it is impossible for us to come to a confirmation of either '$Q_3(c)$' or '$\sim Q_3(c)$' in any degree whatsoever. To give an example, let '$Q_1(c)$' mean that at the space-time point c there is a

person with a certain disease. We suppose that we know symptoms both for the occurrence of this disease as well as for its non-occurrence; hence 'Q_1' is confirmable. It may even be the case that we know a method by which we are able to find out with certainty whether or not a given person at a given time has this disease; if we know such a method 'Q_1' is not only confirmable but testable and moreover completely testable. We will suppose, however, that 'Q_1' is not realizable, i.e. we do not know at present any method of producing this disease; whether or not '$\sim Q_1$' is realizable, in other words, whether or not we are able to cure the disease, does not matter for our considerations. Let us suppose further that clinical observations of the cases of this disease show that there are two classes of such cases, one characterized by the appearance of a certain symptom, i.e. a testable or even observable predicate, say 'Q_2', the other by the lack of this symptom, i.e. by '$\sim Q_2$'. If this distinction turns out to be relevant for the further development of the disease and for its consequences, physicians may wish to classify all persons into two classes: those who are disposed to show the symptom Q_2 in case they acquire the disease Q_1, and those who do not, i.e. those who show $\sim Q_2$ if they get Q_1. The first class may be designated by 'Q_3' and hence the second by '$\sim Q_3$'. Then 'Q_3' can be introduced by the bilateral reduction sentence '$Q_1 \supset (Q_3 \equiv Q_2)$'. The classification by 'Q_3' and '$\sim Q_3$' will be useful if observations of a long series of cases of this disease show that a person who once belongs to the class Q_3 (or $\sim Q_3$) always belongs to this class. Moreover, other connections between Q_3 and other biological properties may be discovered; these connections will then be formulated by laws containing 'Q_3'; under suitable circumstances these laws can be given the form of supplementary reduction pairs for 'Q_3'. Thus 'Q_3' may turn out to be a useful and important concept for the formulation of the results of empirical investigation. But 'Q_3' is not testable, not even incompletely, because we do not know how to decide a given sentence '$Q_3(a)$', i.e. how to make experiments in order to find out whether a given person belongs to the class Q_3 or not; all we can do is to wait until this person happens to get the disease Q_1 and then to find out whether he shows the symptom Q_2 or not. It may happen, how-

ever, in the further development of our investigations, that we find that every person for whom we find 'Q_1' and 'Q_2' and to whom we therefore attribute 'Q_3' shows a certain constant testable property Q_4, e.g. a certain chemical property of the blood, and that every person for whom we find 'Q_1' and '$\sim Q_2$' and whom we therefore classify into $\sim Q_3$, does not show Q_4. On the basis of such results we would state the law '$Q_3 \equiv Q_4$'. By this law, 'Q_3' becomes synonymous – not L-synonymous, but P-synonymous – with the testable predicate 'Q_4' and hence becomes itself testable. But until we are in a position to state a law of this or a similar kind, 'Q_3' is not testable.

This example shows that a non-testable predicate can nevertheless be confirmable, and even completely confirmable, and its introduction and use can be helpful for the purposes of empirical scientific investigation.

Definition 21. If a *sentence* S is confirmable (or completely confirmable) and all predicates occurring in S are testable (or completely testable), S is called *testable* (or completely testable, respectively). If S is testable but not completely testable it is called incompletely testable. If S is bilaterally confirmable (or bilaterally completely confirmable) and all predicates occurring in it are testable (or completely testable), S is called *bilaterally testable* (or bilaterally completely testable, respectively).

Theorem 12. If S is a full sentence of a testable (or completely testable) predicate, S is bilaterally testable (or bilaterally completely testable, respectively).

Theorem 13. If S is a sentence of molecular form and all predicates occurring in S are testable (or completely testable) S is bilaterally testable (or bilaterally completely testable, respectively). – By Theorem 11 and 9 (§ 12).

Theorem 14. If the sentence S is constructed out of testable predicates with the help of connective symbols and universal or existential operators, S is bilaterally testable. – From Theorem 11 and 10 (§ 12).

15. A Remark about Positivism and Physicalism

One of the fundamental theses of *positivism* may perhaps be formulated in this way: every term of the whole language L of

science is reducible to what we may call sense-data terms or perception terms. By a perception term we understand a predicate 'P' such that 'P(b)' means: "the person at the space-time-place *b* has a perception of the kind P". (Let us neglect here the fact that the older positivism would have referred in a perception sentence not to a space-time-place, but to an element of "consciousness"; let us here take the physicalistic formulation given above.) I think that this thesis is true if we understand the term 'reducible' in the sense in which we have defined it here. But previously reducibility was not distinguished from definability. Positivists therefore believed that every descriptive term of science could be defined by perception terms, and hence, that every sentence of the language of science could be translated into a sentence about perceptions. This opinion is also expressed in the former publications of the Vienna Circle, including mine of 1928 (Carnap [1]), but I now think, that it is not entirely adequate. Reducibility can be asserted, but not unrestricted possibility of elimination and re-translation; the reason being that the method of introduction by reduction pairs is indispensable.

Because we are here concerned with an important correction of a widespread opinion let us examine in greater detail the reduction and retranslation of sentences as positivists previously regarded them. Let us take as an example a simple sentence about a physical thing:

(1) "On May 6, 1935, at 4 P.M., there is a round black table in my room."

According to the usual positivist opinion, this sentence can be translated into the conjuncton of the following conditional sentences (2) about (possible) perceptions. (For the sake of simplicity we eliminate in this example only the term "table" and continue to use in these sentences some terms which are not perception terms e.g. "my room", "eye" etc., which by further reduction would have to be eliminated also.)

(2a) "If on May . . . somebody is in my room and looks in such and such direction, he has a visual perception of such and such a kind."

(2a′), (2a″), etc. Similar sentences about the other possible aspects of the table.

(2b) "If . . . somebody is in my room and stretches out his hands in such and such a direction, he has touch perceptions of such and such a kind."

(2b′), (2b″), etc. Similar sentences about the other possible touchings of the table.

(2c) etc. Similar sentences about possible perceptions of other senses.

It is obvious that no single one of these sentences (2) nor even a conjunction of some of them would suffice as a translation of (1); we have to take the whole series containing all possible perceptions of that table. Now the first difficulty of this customary positivistic reduction consists in the fact that it is not certain that the series of sentences (2) is finite. If it is not, then there exists no conjunction of them; and in this case the original sentence (1) cannot be translated into one perception sentence. But a more serious objection is the following one. Even the whole class of sentences (2) – no matter whether it be finite or infinite – is not equipollent with (1), because it may be the case that (1) is false, though every single sentence of the class (2) is true. In order to construct such a case, suppose that at the time stated there is neither a round black table in my room, nor any observer at all. (1) is then obviously false. (2a) is a universal implication sentence:

"(x) [(x is . . . in my room and looks . . .) ⊃ (x perceives . . .)]",

which we may abbreviate in this way:

$$(3) \qquad (x)[P(x) \supset Q(x)]$$

which can be transformed into

$$(4) \qquad (x)[\sim P(x) \vee Q(x)]$$

((2) can be formulated in words in this way: "For anybody it is either not the case that he is in my room on May. . . and looks . . . or he has a visual perception of such and such a kind".) Now,

according to our assumption, for every person x it is false that x is at that time in my room and looks . . . ; in symbols:

$$(5) \qquad (x)(\sim P(x)).$$

Therefore (4) is true, and hence (2a) also, and analogously every one of the other sentences of the class (2), while (1) is false. In this way the positivistic reduction in its customary form is shown to be invalid. The example dealt with is a sentence about a directly perceptible thing. If we took as examples sentences about atoms, electrons, electric field and the like, it would be even clearer that the positivistic translation into perception terms is not possible.

Let us look at the consequences which these considerations have for the construction of a scientific language on a positivistic basis, i.e. with perception terms as the only primitive terms. The most important consequence concerns the method of introduction of further terms. In introducing simple terms of perceptible things (e.g. 'table') and *a fortiori* the abstract terms of scientific physics, we must not restrict the introductive method to definitions but must also use reduction. If we do this the positivistic thesis concerning reducibility above mentioned can be shown to be true.

Let us give the name '*thing-language*' to that language which we use in every-day life in speaking about the perceptible things surrounding us. A sentence of the thing-language describes things by stating their observable properties or observable relations subsisting between them. What we have called observable predicates are predicates of the thing-language. (They have to be clearly distinguished from what we have called perception terms; if a person sees a round red spot on the table the perception term 'having a visual perception of something round and red' is attributed to the person while the observable predicate 'round and red' is attributed to the space-time point on the table.) Those predicates of the thing-language which are not observable, e.g. disposition terms, are reducible to observable predicates and hence confirmable. We have seen this in the example of the predicate 'soluble' (§ 7).

Let us give the name '*physical language*' to that language which

is used in physics. It contains the thing-language and, in addition, those terms of a scientific terminology which we need for a scientific description of the processes in inorganic nature. While the terms of the thing-language for the most part serve only for a qualitative description of things, the other terms of the physical language are designed increasingly for a quantitative description. For every term of the physical language physicists know how to use it on the basis of their observations. Thus every such term is reducible to observable predicates and hence confirmable. Moreover, nearly every such term is testable, because for every term – perhaps with the exception of few terms considered as preliminary ones – physicists possess a method of testing; for the quantitative terms this is a method of measurement.

The so-called thesis of *Physicalism*[32] asserts that every term of the language of science – including beside the physical language those sub-languages which are used in biology, in psychology, and in social science – is reducible to terms of the physical language. Here a remark analogous to that about positivism has to be made. We may assert reducibility of the terms, but not – as was done in our former publications – definability of the terms and hence translatability of the sentences.

In former explanations of physicalism we used to refer to the physical language as a basis of the whole language of science. It now seems to me that what we really had in mind as such a basis was rather the thing-language, or, even more narrowly, the observable predicates of the thing-language. In looking for a new and more correct formulation of the thesis of physicalism we have to consider the fact mentioned that the method of definition is not sufficient for the introduction of new terms. Then the question remains: can every term of the language of science be introduced on the basis of observable terms of the thing-language by using only definitions and test-sentences, or are reduction sentences necessary which are not test sentences? In other words, which of the following formulations of the thesis of physicalism is true?

[32] Comp. Neurath [1], [2], [3]; Carnap [2], [8].

1. Thesis of Physicalistic Testability: "Every descriptive predicate of the language of science is testable on the basis of observable thing-predicates."

2. Thesis of Physicalistic Confirmability: "Every descriptive predicate of the language of science is confirmable on the basis of observable thing-predicates."

If we had been asked the question at the time when we first stated physicalism, I am afraid we should perhaps have chosen the first formulation. Today I hesitate to do this, and I should prefer the weaker formulation (2). The reason is that I think scientists are justified to use and actually do use terms which are confirmable without being testable, as the example in § 14 shows.

We have sometimes formulated the thesis of physicalism in this way: "The language of the whole of science is a physicalistic language." We used to say: a language L is called a physicalistic language if it is constructed out of the physical language by introducing new terms. (The introduction was supposed to be made by definition; we know today that we must employ reduction as well.) In this definition we could replace the reference to the physical language by a reference to the thing-language or even to the observable predicates of the thing-language. And here again we have to decide whether to admit for the reduction only test-chains or other reduction chains as well; in other words, whether to define 'physicalistic language' as 'a language whose descriptive terms are testable on the basis of observable thing-predicates' or '. . . are confirmable . . .'.

16. Sufficient Bases

A class C of descriptive predicates of a language L such that every descriptive predicate of L is reducible to C is called a *sufficient reduction basis* of L; if in the reduction only definitions are used, C is called a *sufficient definition basis*. If C is a sufficient reduction basis of L and the predicates of C – and hence all predicates of L – are confirmable, C is called a *sufficient confirmation basis* of L; and if moreover the predicates of C are completely testable, for instance observable, and every predicate of L is reduc le to C by a test chain – and hence is testable – C is called a *sufficient test basis* of L.

As we have seen, positivism asserts that the class of perception-terms is a sufficient basis for the language of science; physicalism asserts the same for the class of physical terms, or, in our stronger formulation, for the class of observable thing-predicates. Whether positivism and physicalism are right or not, at any rate it is clear that there can be several and even mutually exclusive bases. The classes of terms which positivism and physicalism assert to be sufficient bases, are rather comprehensive. Nevertheless even these bases are not sufficient definition bases but only sufficient reduction bases. Hence it is obvious that, if we wish to look for narrower sufficient bases, they must be reduction bases. We shall find that there are sufficient reduction bases of the language of science which have a far narrower extension than the positivistic and the physicalistic bases.

Let L be the physical language. We will look for sufficient reduction bases of L. If physicalism is right, every such basis of L is also a basis of the total scientific language; but here we will not discuss the question of physicalism. We have seen that the class of the observable predicates is a sufficient reduction basis of L. In what follows we will consider only bases consisting of observable predicates; hence they are *confirmation bases of the physical language* L. Whether they are also test bases depends upon whether all confirmable predicates of L are also testable; this question may be left aside for the moment. The visual sense is the most important sense; and we can easily see that it is sufficient for the confirmation of any physical property. A deaf man for instance is able to determine pitch, intensity and timbre of a physical sound with the help of suitable instruments; a man without the sense of smell can determine the olfactory properties of a gas by chemical analysis; etc. That all physical functions (temperature, electric field etc.) can be determined by the visual sense alone is obvious. Thus we see that the predicates of the visual sense, i.e. the colour-predicates as functions of space-time-places, are a sufficient confirmation basis of the physical language L.

But the basis can be restricted still more. Consider a man who cannot perceive colours, but only differences of brightness. Then he is able to determine all physical properties of things or events which we can determine from photographs; and that

means, all properties. Thus he determines e.g. the colour of a light with the help of a spectroscope or a spectrograph. Hence the class of predicates which state the degree of brightness at a space-time-place – or the class consisting of the one functor[33] whose value is the degree of brightness – is a sufficient basis of L.

Now imagine a man who's visual sense is still more restricted. He may be able to distinguish neither the different colours nor the different degree of brightness, but only the two qualities bright and dark (= not bright) with their distribution in the visual field. What he perceives corresponds to a bad phototype which shows no greys but only black and white. Even this man is able to accomplish all kinds of determinations necessary in physics. He will determine the degree of brightness of a light by an instrument whose scale and pointer form a black-white-picture. Hence the one predicate 'bright' is a sufficient basis of L.

But even a man who is completely blind and deaf, but is able to determine by touching the spatial arrangements of bodies, can determine all physical properties. He has to use instruments with palpable scale-marks and a palpable pointer (such e.g. as watches for the blind). With such a spectroscope he can determine the colour of a light; etc. Let 'Solid' be a predicate such that 'Solid(b)' means: "There is solid matter at the space-time-point b". Then this single predicate 'Solid' is a sufficient basis of L.

Thus we have found several very narrow bases which are sufficient confirmation bases for the physical language and simultaneously sufficient test bases for the testable predicates of the physical language. And, if physicalism is right, they are also sufficient for the total language of science. Some of these bases consist of one predicate only. And obviously there are many more sufficient bases of such a small extent. This result will be relevant for our further considerations. It may be noticed that this result cannot at all be anticipated *a priori*; neither the fact of the existence of so small sufficient bases nor the fact that just the predicates mentioned are sufficient, is a logical necessity.

[33] Compare Carnap [4] §3.

Reducibility depends upon the validity of certain universal sentences, and hence upon the system of physical laws; thus the facts mentioned are special features of the structure of that system, or – expressed in the material idiom – special features of the causal structure of the real world. Only after constructing a system of physics can we determine what bases are sufficient with respect to that system.

(*To be continued*)

Cambridge, Mass.

The Positive and the Logical

BY

ARTHUR F. BENTLEY

ONE is tempted to look upon the positive and the logical somewhat as one looks upon the quick and the dead. Yet the issue is hardly that sharp. Viability has strange possibilities and varied forms, and must often be appraised with an eye directed as much towards the environment as towards the claimant organism. Stretching the application of the word 'viable' to complexes of behavior such as the philosophies and theories of knowledge, we may ask: Is the combination of the positive and the logical in logical positivism viable? And is this logical positivism itself as it stands either positive or logical?

These are interesting questions and no doubt endlessly debatable, though I have no thought whatever of entering the lists of the disputants. I find, however, that a certain amount of light can be thrown upon them by a method as elementary and as humble as that which a high-school student is taught to employ in his first contacts with microscope and bug. We shall try such a method, jot down certain observations in our note-book, and study our reports as they come, with no pretense to deep or soulful understanding.

Logical positivism is an activity or behavior of certain men who call themselves logical positivists;[1] it is their highly characteristic

[1] Logical Positivism is the name most widely current, and it has been approved by Carnap, although he himself more commonly speaks of Logical Analysis and of Physicalism. Other current namings are The New Positivism, Philosophic Analysis, and Logic of Science.

behavior in the region of their most active interest. The logical positivists are alive, and the viability of their theory is dependent upon their continuing life and activity and upon that of such successors as they may have. We may ask whether men of their manner of thought and action are on the way to enjoy successful and widening participation in the great human activities of knowing and of interpreting the known. We may ask whether their behaviors are of a type fitted to maintain themselves in the kinds of environments we may anticipate for their immediate future.

The material that I shall here examine is limited to five contributions to *Philosophy of Science* by Carnap, Feigl, and Blumberg.[2] The inquiry itself is one phase of a wider inquiry undertaken to appraise the present status of the interpretation of science, for which the file of this magazine offered an appropriate assemblage of materials. Various strange situations were found, involving often astonishing abuses of words, even in papers purporting to deal with symbolic logic. Among these the procedures of Carnap and his associates present but a single illustration.

Taking our materials, i.e., the papers above described, just as they come, I apply two tests which I make as simple and as little exacting as possible in the hope that they will not exceed what any reasonable man will think may be required in any serious discussion of science and of knowledge.

Logical. If a procedure is to be regarded as logical, its advocate may not within the limits of any one essay declare a proposition to be fundamental, use it as such, and then later on casually introduce a protecting clause to the effect that the proposition itself may of course be defective or perhaps even false. Nor, when he makes an assertion, may he follow this later within the same essay by its contradictory, without explaining the transition.

Positive. If a procedure is to be regarded as positive, it must employ materials of investigation and adopt attitudes of inquiry comparable to those used by the successful sciences of our era—

[2] R. Carnap, "On the Character of Philosophic Problems" (I, 5), "Meaning, Assertion and Proposal" (I, 359); H. Feigl, "The Logical Character of the Principle of Induction" (I, 20), "Logical Analysis of the Psychophysical Problem: a Contribution of the New Positivism" (I, 420); A. E. Blumberg, "The Nature of Philosophic Analysis" (II, 1).

the ones we most generally recognize and describe as positive with respect to the form of the knowledge they offer. It may not merely steal the use of a popular name. It must, in particular, exclude the use of all fairies, spooks, and spirits, whether fair or foul.

Lethal. Where a construction shows itself defective by both tests, and where, moreover, the defects of the first type can be directly traced to improper procedures of the second type, we may fairly regard the whole construction as an inadequate form of human behavior, lethal in its type, and doomed to disappear.

It may as well be stated at once that the evil spirit we shall find at work in the selected papers before us is one that manifests itself in the use of the word 'concept.' The 'concept' as our logical positivists employ it proclaims a reserved right on their part to "really mean" something different from what they say. Their 'concepts' may thus be taken as marking gaps, or holes, in their language—places where words fail and where the expounder waves a magic wand to assure us nevertheless that all is well.

As to the status more generally of this word 'concept,' we may remind ourselves that it derives from the old language of 'consciousness' and 'mind,' and that all formal definitions of it are in terms of some 'mental' that is a 'not-physical.' Its peculiarity is that it is in common daily use by men who have long since abandoned all belief in any such 'mental'; they take it for granted, apparently without once asking themselves what expressly it is that they are talking about. They face some kind of a 'factual' situation; they find the word 'concept' not only convenient but eminently respectable; they proceed to use it. The result is that 'concept' becomes a weasel word in philosophy's service far more efficient in deception than any word the amateurs of the politicial party platforms have evolved. One can find it with as many as half a dozen distinguishable applications in a single paper. While indicating sometimes a "bit of consciousness," or "a thought," or an assumption of "precise meaning," at other times it can be directly replaced in the text by the word 'word' itself. It may offer any desired combination of, or stage of transition between, 'mind' and 'word.' It may cover sentence, proposition, theory,

or doctrine. The part it plays for logical positivists is therefore not unique, but merely symptomatic of a disease of language, wide-spread in our era.

The purpose of *Philosophy of Science* as announced by the editor in his introductory statement (I, 1)[3] was to aid in the clarification of philosophy and science, and perhaps in their unification through such typical projects as "the examination of fundamental concepts[4] and presuppositions in the light of the positive results of science, systematic doubt of the positive results, and a thoroughgoing analysis and critique of logic and of language." Upon all of these questions Carnap and his associates have had much to say, and Carnap's paper "On the Character of Philosophic Problems" was well indicated for the leading place in the first issue. It was Carnap's emphatic assertion, his main thesis, that by "purely formal procedure" the logic of science can "finally arrive at the answering" of all connotative questions (I, 9). This "connotative" of Carnap's (in the German, *inhaltliche*) covers all issues of what we most generally describe as 'meanings,' since he places the *inhaltliche* in sharp opposition to the *formale*, and expressly defines the latter as covering the study of symbols and propositions "*without reference to the meaning*" (I, 9). In plain everyday language Carnap asserts that all questions of meaning can be answered without reference to meaning; or, in more erudite language, that a symbolic procedure stripped of all implications of meaning will be humanity's final resort for solving all problems of the import of knowledge.

Remember now that we are to examine only the materials immediately on the table before us. If we do this in a highly "formal" way, Carnap certainly should not be the man to object. It does not matter to us what Carnap writes elsewhere nor how he may modify his construction from year to year; our only concern is the integrity of the particular specimen of work before us. It is as if the boy in the laboratory studying the bug should dis-

[3] References in the text are to volume and page of this magazine.

[4] 'Concept,' of course, may be honorably used. In the present instance it stands for "that which is written about philosophy and science," while the 'fundamental' and the 'presuppositions' serve to concentrate attention on those regions of the wide field that seem of greatest significance for purposes of clarification and unification.

regard all speculation as to its place in Nature's Mighty Scheme, and confine himself to such simple observations as how well its wings are fastened to its body, and whether its alimentary canal is open at both ends.

We begin by applying our first simple test which we labelled with the word 'logical.' And here we can follow a lead already taken in the pages of the magazine by John Dewey. Dewey read Carnap's paper and at once asked him certain pointed questions which, to anyone interested in the simpler "facts of life," have devastating import (I, 237). Carnap gave no sign of attention either to facts or import, but instead spun the web of his reply (I, 359) in a manner much nearer the medieval than the modern. Since questions and answers with accompanying page citations are directly available to all readers who have this magazine in hand, I may be permitted to paraphrase and condense.

Dewey first cited Carnap's assertion that by "purely formal procedure" the logic of science can "finally arrive" at the answering of all *inhaltliche* questions. He then set side by side with it three of Carnap's further statements as follows: mathematical meanings derive from empirical considerations; statements of fact are synthetic[5] (that is, neither analytic nor contradictory, but involving definite truth or falsity); the very selection of a linguistic form on which to rely (such as *formale* or *inhaltliche* itself) requires an empirical decision.

Dewey was very gentle and considerate in his comment. He merely said that Carnap's position "is not clear to me beyond a certain point." But he proceeded to show that among Carnap's tabulated illustrations, seventeen in number, the *formale* expressions were apt to be the ones with precision of 'meaning,' while their *inhaltliche* alternatives were too often either meaningless or just vaguely ambitious. And beyond that he was surprised to find Carnap at the end of his paper, after having built entirely on a severance of *formale* from *inhaltliche*, calmly remarking that

[5] Carnap gives his distinction between "analytic" and "synthetic" on p. 53 of *Philosophy and Logical Syntax*, 1935, a small book covering more fully the ground of his two contributions to *Philosophy of Science*. "The terms," he says, "have already been used in traditional philosophy; they are especially important in the philosophy of Kant; *but up till now they have not been exactly defined*" (italics mine).

there was nothing fundamental at all about his distinction between the two approaches.

Carnap in reply admitted a lack of clarity in his fifteen-page essay. He said this was because "concepts" were necessary which were far too technical for use in any such place. Even in personally addressing Dewey he was compelled to fall back upon descriptions which he admitted to be "somewhat inexact." He "distinguished" three sorts of sentences—"real object-sentences," "pseudo-object-sentences," and "syntactical sentences"; the first of which deal with "extra-lingual objects" (elsewhere called "empirical matter"), the second "deal seemingly" with them (although actually with "the lingual signs of those objects"), while the third "deal with lingual expressions." He assured Dewey that the whole distinction between *formale* and *inhaltliche* concerned only those sentences "which deal with language," as though that were extenuation enough for factual falsity.

We have here an authoritative sample of Carnap's "logical analysis. The question is whether it is entitled to be called 'positive'? More particularly we may ask: Does the array of sorts of sentences which Carnap "distinguishes" offer us a *positive* classification of linguistic materials in any sound sense of the word 'positive'? How far can we outline 'operations' or establish 'facts' to correspond? How far can we so much as attempt this?

The first difficulty we find lies in the criteria of classification, one sort of sentence being identified with respect to language, another with respect to what *is* not-language, and a third with respect to what *seems to be* not-language. We have here 'sentences,' 'objects,' and 'seemings' used for tests in classifying sentences themselves, and we may expect much confusion in the search for applicable 'operations.'

Next to attract attention is the peculiarity of the hyphenation. Order the groups as follows: syntactical sentences, real object-sentences, pseudo-object-sentences.[6] Observe this closely. You

[6] This manner of hyphenation is not accident or error in hurried magazine publication; it is repeated in the book just cited, p. 61 (table and accompanying text). A typical effort to throw light on the "pseudo-object-sentences" is as follows: "We may call the quality of being a thing-designation a *parallel syntactical quality* to the quality of being

will find that it would take several pages to bring out all the issues the hyphens evade. The "syntactical" is not balanced against the "object" in a first stage of classification, nor the "real" against the "pseudo" in a second stage; it is not even clear which stage has precedence; no 'genus' can be identified for the three 'species.' The hyphens are disingenuous; they are Ethiopians—'conceptual' Ethiopians, so to speak—in the linguistic wood-pile. They mark, not classification, but casuistry.

So much for the "syntax" of Carnap's procedure. Inspect it now rather for its "content." He fails to show that his test, i.e., what the sentences are 'about,' is dominant for his problem; other important possibilities for ordering are available, but we may pass this over. Most troublesome is the fact that he posits his "real object-sentences" and his "syntactical sentences" in such conceptual purity that it would be difficult for him (personally I would say impossible) ever to exhibit a specimen of either. All the interest of inquiry lies in the intermediate territory, and here the procedure is deliberately that of facing both ways. The reader will note (a) that instead of clearing the presentations from the difficulties of the 'seeming' and the 'actual,' this complexity is expressly maintained; (b) that the phrase "material mode of speech" is here applied because of its verbal suggestion of empirical matter on the one side and of formal mode on the other; and (c) that the parallelism of "being a thing" with "being a thing-sign" is another bit of prestidigitation between matter and mode. In effect Carnap arranges a logical key-board to be played by both hands in the pious wish that in time the right hand may come to 'know' what the left hand is doing. Right hand plays language-language with a permitted shift from a formal to a material mode. Left hand plays thing-language with a set of pseudo-thing variations. The organ stop marked "parallel syntactical quality," when pulled out at the right moment by the right man, is supposed to make "logical analysis" become "positive."

One further conflict in Carnap's statements may be noted,

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a thing" (*Ibid.*, p. 62). The objection here is not nearly so much to the highly formal terminology Carnap uses, as it is to the heavy burden he imposes on the wholly informal term "quality."

though its nature is much like what has preceded. In connection with his assertion that all science is language in the sense of a system of propositions (I, 8, 14), he maintains that language is a "calculus" and that its formal rules of procedure are "mechanical operations" (I, 10). Then later he remarks that "mere calculating about" is not enough (I, 19). Without the first attitude he could not set up his main thesis, nor undertake his proposed sentence-classification. Without the second he could get nowhere after setting his thesis up. The status is this: A certain "parallelism" of ancient lineage is disclaimed and expelled. Later, however, hobbled and veiled, it is admitted through the back door. We are told that this does not matter, for now it is fully under linguistic control. "The distinguishing of the formal and the material modes of speech concerns only those sentences which deal with language" (I, 360). There it is justified, there it rules, there it characterizes "logical analysis"; otherwise, incidentally, it is false.[7]

Carnap, of course, himself sees unity and coherence in all this; and we must seek the locus of his confidence. But first observe how powerful that confidence is. Dewey had asked him a further question, one concerning the distinction he had set up between "assertions" and "proposals." He could not answer this question, but he assured Dewey nevertheless that the distinction was sound, and that his criterion could be applied, "true or false," to any sentence, "provided of course the sentence S_1 as well as the whole connection to which it belongs is given in a perfectly correct and complete form" (I, 360). Here is a requirement which a disciple may no doubt accept, but which to the ordinary inquirer midway in the passage of our lives may perhaps seem an ideal just a shade too bright and good. Nevertheless it exhibits Carnap's reliance, his typical reliance, in all his work.

Proceed, Carnap tells us in effect, with an infinite series of regressive "meanings," being very careful at every stage to strip yourself of factual or experiential debasement; make every "meaning" a "symbol," and purify the symbol so thoroughly of

[7] Compare Blumberg's blunt assertion (II, 3): "There are two basic groups: assertions of syntax and assertions of fact. This dichotomy follows from the nature of language."

all meaning that you become sure it has "no meaning at all"; then in the end you will secure final and complete control of all "meanings" and of all knowledge. He casts this "proposal," this device, up against the logical sky in the form of a huge calculating machine. He does not, however, assert it to be independent and absolute perpetual motion. It has to be produced in some way, and it evidently has some kind of an operator. What operator? His operator is in plain view, even though its head is buried in the verbal sands. It is what in mathematical analogy we may call the "point-soul," or in physical analogy the "particle-mind."[8] It is that from which everything starts and to which everything comes back, the limit of all possible statement. From it arise the verbal flights through which knowledge is to be acquired and appraised. In it lie the attributes of truth on which Carnap relies even when all his words and sentences prove false. It alone is the authority whereby the logician may dare to soar above all those situations which currently we regard as experientially factual or as approximations to fact.

This operator of Carnap's possesses a tool—a magic tool—the 'concept.' The 'concept' does everything the operator wants it to do. When he wishes, the concept garbs itself as a 'word'; then the system deals with 'language.' When he wishes otherwise, then the concept transforms itself into 'meaning,' and the system runs far away from the 'language' of its primary confession of faith. By means of the 'concept,' itself a word, every word is empowered to take shifting values—concept, meaning, symbol, proposition, language, science, even "no meaning at all." The intention of the 'concept' is pure, and everything must come home in the end to roost on the wrist of its master, the point-soul; then why bother with minor temporary inconsistencies, or tests, or practical 'operations' en route?[9]

[8] The "asperities of a marriage between Empiricism and Solipsism" are noted by the translator of Carnap's essay "The Unity of Science," p. 12.

[9] Matter-of-fact examination of Carnap's 'concepts' of *Inhalt*, *Gehalt*, and *Folgerung* will give other interesting examples of evasiveness in conceptual procedure. *Folgerung*, is said to be "quite different" from material implication, since the latter depends only on truth-values while the former "is not quite determined by the truth-values" (I, 11). *Gehalt* is "a purely formal concept"; nevertheless it "corresponds completely to what we

Carnap and his followers take advantage, nevertheless, of whatever opportunity opens to associate their procedure with Bridgman's operations; at times they draw support from him, at others they allot him approval as an earnest seeker, though of the lower caste of science.[10] Bridgman makes 'concept' his key-word and brings in a credo of the solipsistic self like a refrain to his writing. The difference between Bridgman and the logical positivists is, nevertheless, almost antipodal. What Bridgman *does* with concept and self is to bring them under harsh control by physical operations of the most advanced laboratory type. What the logical positivist *does* with them is to run to them for help whenever he gets into trouble.

Point-souls of the type Carnap uses are largely discredited in more recent psychological and sociological inquiry, and they are wholly discredited by the *phenomenon* of Gestalt; but they are so deeply embedded in the German language with its terminologies of *Geist* and *Seele*, *Vernunft* and *Verstand*, that linguistically they dominate there beyond all suggestion of observational search or test. Even the experimental work of Gestalt is most often construed in one or other of their forms; even the Marxian is still bound. Carnap is of their inner faith. The last stronghold of such point-souls is in logic, and there the issue will be fought out. What concerns us of course is no question of 'reality,' whether such souls actually 'exist' or not being of no direct importance. We are concerned strictly with the phenomena before us, namely, those linguistic behaviors in which point-souls whether by direct assertion or implicit use are prominent factors.

We have one more astonishing observation to make with regard

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mean when we (in a vague manner) are accustomed to speak of the 'meaning' (*Inhalt*) of a proposition" (I, 12–13). Carnap's creed of conceptual infallibility has been expressed by him as follows: "Die Begriffe sollen aus gewissen Grundbegriffen stufenweise abgeleitet, 'konstituiert' werden, so dass sich ein Stammbaum der Begriffe ergibt, in dem jeder Begriff seinen bestimmten Platz findet."; *Der logische Aufbau der Welt*, 1928, p. 1.

[10] *See* Feigl, I, 20; I, 424. Compare also two papers in *Journal of Philosophy*, 1931, one by Blumberg and Feigl, p. 281, and one by Blumberg and Boaz, p. 544. In the latter we are told that Bridgman calls his theory of meaning "operationalism," that it is "a technique for building concepts," and that as such it "can be shown to be a special case" of logical positivism. For these writers it is "definition" which "formulates operations."

to this point-soul of Carnap's as it actually performs. As itself concept and bearer of concepts, and as sole point of ultimate reference, it refuses to remain a point. It splits. It becomes a two-faculty soul of a type so ancient and elsewhere so discredited that it is astounding to find it surviving and strongly entrenched in a recent logic. The proposition about the "two languages" which the logical positivists make and all their distinctions of data and constructs arise in this way. Our own discussion has covered this situation in a different linguistic form, and it will be enough here to give footnote reference to other comments upon it in the pages of *Philosophy of Science*.[11]

If we take Carnap's *formale* and *inhaltliche* out of the rigid 'conceptual' forms in which he propounds and contrasts them, and if we examine them in everyday life where although Carnap recognizes their union he has no 'logical' use for it, we find our separations of them all temporary and provisional, and the return always necessary to their combined behavioral process. We forever strive to formalize our expressions as fully as we can, and to succeed we must limit. But also we strive forever to give our expression as rich, as complete, as factual a reference as we can. And here each success means a new destruction of the old 'conceptual' identifications; it means the widening and organizing and systematizing of our knowledge. Carnap has every right to make his split, and to make it as sharp and rigid as he can. But this is the right to postulate and to test. He cannot expect to convince the world by fiat or by spiritist appeal or by glowing picture of ulti-

[11] Schilpp accuses the logical positivists of refusing to permit any analysis of the concept of the 'given,' even though it is fundamental in their construction, and even though the analysis of fundamental concepts is declared by them to be the sole legitimate work of philosophy. "The 'given'," he says, "is the point where the logical positivists turn mystics" (II, 129). Weinberg (II, 387) from a different behavioral base, with wholly different objectives, and by the use of professionally logical techniques, attacks a similar problem in the case of Wittgenstein's logical simples; this criticism is pertinent since Wittgenstein's attempted rigor is the historical source from which the degeneracy of logical positivism stems. Stevens (III, 100), speaking for psychologists, finds the dichotomy between data and constructs artificial. Malisoff (II, 339) notes the "black magic" in the "intersubjective" of the positivists (for a case of its use, *see* Feigl, I, 433 ff). Kattsoff finds Carnap's construction "misleading and doomed to failure" (III, 70) though this is mainly on the ground that it is a two-valued logic. Other comments will be found at I, 325, 331, 482; II, 256, 273, 295, 364; III, 124.

mate ideal success. He must work within the stream of knowledge and within that portion of the stream within which we find ourselves today. It is this which can and will guarantee factual orientation on the one hand and heightened consistency of expression on the other—the two hand in hand.

The papers of Feigl and Blumberg are strikingly similar to Carnap in the characteristics we have been considering. Feigl's logical positivism results in verbal anarchy. Blumberg surpasses him by expressly recognizing that such is the outcome, while at the same time holding as firmly as ever to the basic principles of the creed.

Feigl proposes to exhibit "the strict identity of the 'mental' life with certain processes in the 'physical' world" and to show that this is "not a matter of belief . . . but a truth capable of logical demonstration" (I, 420). His confidence is so great that he declares (I, 444) that if his analysis leaves any riddle unsolved "then it is an insoluble one that cannot be expressed as a significant question in any logically legitimate language." His procedure, he says, will be by way of "a metaphysically neutral logical analysis of fundamental concepts" (I, 420). He uses an "epistemological subject"; he declares this to be a necessity for logic since it is the only source of answer to the question: "How do we justify our factual assertions?" (I, 428); nevertheless he protests vehemently all the time that it is a fiction.[12] He disclaims all dogma that makes such a subject "individual," but he says that in use it inevitably becomes "individual," and that in no other form is it possible. If this is "solipsistic," says Feigl, it is "only in the *methodological* sense, which is metaphysically strictly neutral" (I, 429). In other words Feigl exorcises metaphysics, but goes right ahead with "fundamental concepts," "epistemological subjects," isolated "individuals," and whatever else he wishes, all under the name of "logic" and of "method." He gets rid of some of the spoiled chicks of metaphysics, and makes a great virtue of it,

[12] Another case in which Feigl deliberately adopts a fiction as a "necessity" for logic is that of "form" and "content" (I, 440). This, he says, is a radical dualism, it is the only radical dualism he acknowledges, and it is a "very important but nevertheless artificial philosophical abstraction."

but he builds a gilded temple over the metaphysical incubator, and proclaims a priestly rule in the name of the logical and of the positive.

In various other ways the dangers of the 'conceptual' procedure are demonstrated in Feigl's pages.[13] He begins, for example, by declaring "perceptions, thoughts, desires, dreams" to be "empirical existences"—data that all trained people can agree upon under the very same tests that are applied to "tables, trees, people, stars. . . ." etc (I, 423). He winds up by turning the "physical" and the "mental" into two separate "symbolic systems," "universes of discourse, characterized by their logical syntax" (I, 442). In the course of his development for these "two languages," both under the aegis of a "language about languages," he arrives at the following sentences which we may cite without further comment:

"The physical language is *retranslatable* into the language of data" (I, 430).

"The physical language is not only translatable into the language of data but also vice versa" (I, 435).

"Mutual translatability means nothing but identity of structure" (I, 436).

"The two languages have incompatible syntaxes . . . they are structurally completely disparate" (I, 439).

Blumberg uses the title "Philosophic Analysis" for his form of logical positivism. I will set down its characteristics as literally as I can in the immediate terms of its presentation. He tells us that we must say "verify" (but if we do not like to say "verify," then we may say "confirm"; II, 4). If we "confirm," however, we must be prepared to admit all factual statements as "at least theoretically capable of verification," which means that such statements must permit themselves to be "completely translated" into "atomic propositions" (but if we do not believe in "atomic propositions," then we may "take the terms 'simple,' 'ultimate,' 'basic,' and 'atomic' in a relative and not an absolute sense"; II,

[13] In another paper Feigl comparably handles "the principle of induction" of which he says "as long as there is knowledge . . . it will be its inescapable guiding maxim" (I, 28). One may note as characteristic his frequent assertions of what *must be*, as, pp. 26–7, for the "ground floor of knowledge," for atomic propositions, and for the possibility of psychology; his certainty that the principle of induction is to be explained by a frequency theory of probability, even though the latter is not yet achieved (I, 25); and his many struggles to reformulate Bridgman's "operations" through un-Bridgmanlike terms such as prescript, prescriptive rule, regulative maxim and the principle of a procedure (I, 20, 27).

5).[14] Proceeding thus he quickly finds "true definition" and "correct usage" in close quarters, but it is wholly wrong to attempt to equate them; instead we may carefully say: "the analysis which yields the definition will be *correct* if the definition does actually render the usage examined" (II, 7). All this, however, is merely "description" and philosophers must rise to "prescription," though when they get there they have nothing to rely on except "considerations of utility, convenience, and aesthetic value" (II, 7); nevertheless they may in the end come to "speak of *the* meaning of a sign, *the* correct analysis of an assertion." While all this is "difficult in practice" it is "simple in theory" and will finally result in a substitution of the "concrete, practical" for the "vague, exalted, mystical" (II, 8).

The above somewhat astonishing outcome of "philosophic analysis" flows professedly from "the nature of language" which involves a "fundamental distinction" (II, 3) between statements of syntax and statements of fact. The parentage of this linguistic base itself is in point-soul and concept. Blumberg does not use the word 'concept'[15] in his paper, and abstention here, unless it is a minor treachery, is always a favorable sign. His stress is heavy on the behaviorally phrased inquiry: "In what ways do people use a certain sign?" His outcome is close to immolating point-soul and concept alike, and by that test we may say that it offers something that a normal scientific investigator would regard as a fair, even though still crude, starting point, for his inquiry. One may even anticipate on his own showing that he, or some other inquirer in similar state, will soon strip off the apparatus of logical positivism in order to undertake direct and positive investigation.

It is hardly necessary for me to say that with the great objectives of the logical positivists—the expulsion of metaphysics, and the development of a linguistic frame for appraisal and organization—I am in the fullest sympathy. What is criticized in the present paper is solely some of the detail of workmanship.

Paoli, Indiana.

[14] Blumberg's statement (II, 5) as to what "completely analyzed" would mean in a relativistic sense "at-a-given-level-of-atomicity" makes good collateral reading. Interesting also is the way 'sign,' 'signify,' and 'significance' are used (II, 4).

[15] He uses 'notion' once, and otherwise distributes the work of the word between 'term,' 'assertion,' 'sign,' 'meaning,' 'usage' and 'definition.'

Physical Continuity

BY

FREDERIC B. FITCH[1]

MATHEMATICAL continuity, in the technical sense, is a precisely definable mathematical notion which refers to certain properties of numbers and number sequences. The continuity of the physical world, on the other hand, is rather different from mathematical continuity, since it is a directly experienced attribute of nature and does not require, for being understood, any mathematical theory of properties of numbers.

What is the relation or relevance of mathematical continuity to physical continuity? Is there any such relevance, or is the use of the terms "continuity" for both notions merely a linguistic accident? An answer to these problems would seem to entail some explanation of why equations involving mathematically continuous functions are highly convenient for describing the physically continuous world, and why the "infinitesimal" calculus is so useful in a world where the infinitely small is never found.

The most significant fact about mathematical continuity is its dependence on the mathematical theory of limits. Thus, roughly speaking, a geometrical figure is continuous if (1) every limit point of the figure is a point of the figure, (2) every point of the figure is a limit point of the figure, and (3) the figure does not consist of two figures which have no limit points in common.[2]

[1] Sterling Fellow, 1935–36. During 1934–35, when this paper was first drafted, the author held a Du Pont Research Fellowship at The University of Virginia.

[2] *Cp.* G. T.Whyburn, "*On the structure of continua*," Bul. Am. Math. Soc., Vol. 42 (1936), pp. 49–73.

By a "limit point of the figure" is of course meant a point to which converges some sequence of points of the figure. Since any geometrical figure can be viewed as being simply the aggregate of points that compose it, and since every point in an n-dimensional space can be viewed as an ordered set of n real numbers, it follows that any geometrial figure can be treated as being a purely numerical construct.

In contrast to such an ideal numerical construct (for example a sphere of unit radius), let us consider an actually existent material sphere of one centimeter radius. Suppose that this sphere consists of steel and that, under certain constant conditions of temperature and pressure, repeated measurements reveal that it is, within the margin of experimental error, a perfect sphere. What, now, do we mean by saying that such a sphere is continuous? Or rather, in what sense could such a sphere be discontinuous? If we found upon examination that the sphere really consisted of several parts perfectly fitted together but separable, we might assert that in this sense the sphere was discontinuous. Or knowing that the sphere consists of protons, neutrons, and electrons, we might assert that it was for that reason discontinuous. If the sphere were not made of steel but of some absolutely unbreakable substance, then the sphere would be continuous in a very definite sense. But no such unbreakable spheres are known to exist.

Now there can be no doubt that our steel sphere possesses *some* sort of continuity. For Democritus this continuity would not be intrinsic to the sphere but would be an illusion due to the confusions of the realm of sensation through which the sphere is perceived by an observer. For him the real sphere would be a mass of atoms. For this reason he holds that all ideal geometrical figures, because of their intrinsic continuity, exist only in the sense world and have no existence in the "real" world of his material atoms. It is, of course, hard to understand how he can then ever speak of the shapes of his atoms; but his plan to relegate continuity to a region of indistinctness and confused merging suggests that indistinctness may well be a fundamental clue to the nature of physical continuity. Any object, indeed, is *dis-*

continuous in so far as it consists of sharply differentiated parts. The steel sphere itself, unless it is broken to pieces, does not consist of sharply differentiated parts; and this is all we mean in saying that it is continuous.

An obvious objection is that the sphere *really does* consist of sharply differentiated distinct parts, i.e., molecules, atoms, protons, neutrons, and electrons. But in reply it can be said that the molecules, atoms, etc., are best considered as electromagnetic fields merged together according to certain laws and existing in certain partially indefinite "states." Only as thus merged do they constitute the steel sphere. They are not separate and sharply distinguished parts of it. We have no way of distinguishing one atom of the sphere from another. We know only the total or average effect of millions of them. The continuity of the steel sphere is thus, in a sense, its homogeneity. Yet we must admit that the sphere is not *completely* continuous, because it is at least *capable* of being broken into distinct parts; and individual atoms or electrons that originally constituted it might even be isolated from it. But in its spherical state that steel sphere has a high degree of homogeneity and therefore a high degree of continuity.

Even if the sphere is broken into sections or divided into atoms, there always remains a certain ultimate continuity in each part. This fact is expressed in the famous Indeterminacy Principle, which requires that the product of the momentum and position of a particle can never be meaningfully determined with greater accuracy than the magnitude of Planck's quantum of action. This leaves, at least in the dimension of action (momentum times distance, or energy times time), an ultimate and irreducible homogeneity and continuity. The quantum of action is generally supposed to indicate discontinuity in nature. But if we nowhere came upon an objective limit to detailed analysis, there would be no ground for asserting that nature anywhere possesses objective homogeneity and togetherness. We would, on the contrary, be driven to suppose, with Democritus, that continuity is somehow illusory.

Physical continuity is, indeed, simply the objective indeter-

minacy[3] or "elementary diffusion"[4] of nature. The discovery of the quantum or "atom" of action constitutes, paradoxically enough, the discovery of objective continuity in nature. The only sorts of quantities that can be continuous and yet analysable into a finite number of parts are those quantities which can be quantized in the sense that action (and perhaps length[5]) is quantized, for otherwise there would be no merging between the parts of such quantities. This "merging" means simply that more exact analysis is meaningless; in other words it means continuity and "fusion."

The paradoxes of Zeno take on a novel aspect in the light of this theory of physical continuity. The first paradox, for example, breaks down if quanta of length are permitted. It is no longer necessary to traverse half a quantum of length before traversing the whole quantum of length, because there would be no half-quantum of length.

It is to be noted that this theory of continuity rejects the validity of the contradiction usually assumed to exist between atomism and continuity. On the contrary it maintains that continuity necessarily involves a kind of atomism. The atoms, however, are such that it is *meaningless* to divide or cut them, rather than impossible because of their solidity. This meaninglessness is expressed, for the quantum of action, by the indeterminacy principle. If the universe is really somehow built out of these atoms of action (and perhaps also out of atoms of length, etc.), then these atoms are indeed the ultimate units in terms of which all measurements must be made, and clearly we could not measure one half of one of them by using others as units of measurement. All that could ultimately be done is to count integral numbers of them and study the structures into which such integral numbers of them enter constitutively.

[3] This does not imply any indeterminateness of causal connection, but merely an indeterminateness of the states between which determinate causal connection may hold. *Cp.* F. S. C. Northrop, "*The philosophical significance of the concept of probability in quantum mechanics*," Philos. of Science, Vol. 3 (1936), pp. 215–232.

[4] I am indebted to Prof. H. Margenau for this term. See his "*Methodology of modern physics*," Philos. of Science, Vol. 2 (1935), pp. 48–72, 164–187, esp. 174 ff.

[5] *Cp.* L. Landau and R. Peierls in Zeits. f. Physik, Vol. 69 (1931), pp. 56–69.

It is often supposed that the theory of light offers a case of the alleged contradiction between atomicity and continuity. But the hypothesis here suggested would maintain that the "atomicity" of light and its continuity are different aspects of the same situation. Light is transmitted only in units of Planck's quantum of action simply because light *is* a continuous physical process. The reputed contradiction arises only if we try to employ a Democritean theory of solid, unbreakable, definitely determined atoms of material substance. Atomism in the new sense implies continuity instead of contradicting it.

With this view of the nature of physical continuity before us we must now inquire why it seems to be necessary to employ *mathematically* continuous functions in equations that express *physically* continuous processes. This question is especially interesting in view of the fact that mathematical continuity requires an employment of the real number system (including square roots of prime numbers, etc.), whereas all physical measurements seem to be in terms of rational numbers (i.e., fractions, ratios between integers). The laws of physics are most easily stated by means of second-order differential equations, involving a double application of the rigorous notion of limit (in terms of infinitely decreasing quantities), whereas all experiments verifying such laws are valid only within some margin of error and therefore can have nothing to say about the ultimate limits that are approached by infinitely decreasing quantities.

The answer seems to be that all the laws of physics could be stated in terms of the rational numbers and without recourse to the infinitesimal calculus and the theory of limits, but the laws thus stated would be unnaturally complicated. These complications would arise in several ways. In the first place, the system of rational numbers does not contain such numbers as $\sqrt{2}$ or π. Thus if we used only rational numbers, there would be no uniquely determined number which we could assign to the circumference of our steel sphere, knowing that within the experimental margin of error its radius is one centimeter. There would, on the contrary, be an infinite set of numbers which would not differ from 2π by more than the margin of error; but there would be no satisfactory way of giving preference to one of these rather than to

another. If we use the system of real numbers, on the other hand, there would still be an infinite set of numbers at our disposal, but among these numbers there would be a number, namely 2π, which would especially well serve our purposes for designating the length of the circumference. No matter what steel sphere of radius one centimeter is under consideration, we can be sure that no error will follow from assigning it a circumference of 2π centimeters. But more than this, we know that no matter how small the margin of error may be, the number 2π can serve, within this margin, to designate correctly the length of the circumference; and no matter how large a sphere we choose, the circumference will, within *any* assigned margin of error, be correctly given as 2π times the radius. This is all because 2π is determined by means of a process of limits which takes account of any margin of error, however small. Accordingly, by using the system of real numbers and the theory of limits we can assign a number to the circumference of a sphere, whose radius we know, without inquiring as to the actual size of a relevant margin of error or indeterminacy, or even as to the size of the sphere itself. No number thus independent of the margin of error could be found if we were limited to only rational numbers.

If we are to accept the indeterminacy principle, then we must admit that nature is itself somehow permeated with "margins of error" or "margins of indeterminacy" which are more objective and fundamental than the "experimental margin of error" of some specific physical experiment. This diffuseness and microscopic merging of parts that exists in physical nature is the explanation both of the fact that there always exist experimental margins of error and of the fact that we find it convenient to employ the real number system and the method of limits in order to minimize discrepancies that margins of indeterminacy tend to introduce. The differential and integral calculus is, after all, merely an elaboration of the theory of limits. The physical significance of the use of the calculus to express relations among *physically* continuous quantities is that the calculus offers a form of expression that is valid no matter how small the margin of indeterminacy is, relative to the objects or processes studied.

To return to the steel sphere, let us suppose that we place a flat

steel surface against it. The effect is expressed mathematically by saying that a plane is tangent to the surface of the sphere. The slope of the flat steel surface (relatively to some coördinate system) is found by experiment to vary with the place at which it contacts the steel sphere. For any given point of contact various values could be assigned to the slope, all within whatever margin of error is assumed; but not all these values would be valid if another margin of error, smaller than the former, is assumed, or if a larger sphere is used. By taking the equation of the *ideal geometrical sphere*, however, and differentiating this equation properly, a certain unique value for the slope at a given point can be selected which can be used no matter how small the margin of error may be or how large the sphere. This is because mathematical differentiation is defined in terms of a limiting process which considers an indefinite decrease in the margin of error. The relevance of mathematical continuity to physical continuity arises from the fact that physical continuity is ultimately a sort of indeterminacy and homogeneity introducing into our equations error factors that can be systematically minimized only by means of the method of limits of the theory of mathematical continuity.

In conclusion it may be well to point out that the foregoing theory of physical continuity has much in common with some of the views of Bender[6] and Malisoff.[7] Bender's "statistical mean", for example, is associated by him with the continuous laws of classical physics, and this "mean" really seems to arise as the joint statistical effect of the mathematically non-continuous "relative scale quanta." In a somewhat similar way, we have been holding that mathematically continuous functions are adapted for treating the total effects of various sorts of *physically* continuous (i.e., quantified) phenomena, for the reason that such functions minimize discrepancies arising from the presence of quanta. Thus mathematically continuous functions are suited for statistical

[6] Wm. Bender, "*The method of physical coincidences and the scale coordinate*," Philos. of Science, Vol. 1 (1934), pp. 253–272.

[7] W. M. Malisoff, "*An examination of the quantum theories. IV*," Philos. of Science, Vol. 2 (1935), pp. 334–343. See also, *loc. cit.*, p. 335, references to Seeger and Lindsay; also, A. Einstein, "*On the method of theoretical physics*," Philos. of Science, Vol. 1 (1934), pp. 163–169, esp. p. 169.

generalizations which disregard those variations which are of the order of magnitude of "relative scale quanta."

The justification for the use of physical postulates employing mathematical continuity is, from one standpoint, simply that such postulates give rise to verifiable theorems and do not give rise to theorems which are known to contradict experimental observations. But, as Malisoff[8] rightly maintains, "Physical meaning has been sought as a rule in incidental products of mathematical development, whereas it should be sought more assiduously at the fountainhead, in the very postulates, no matter how abstract and forbidding they seem." In a sense we have indeed been attempting to give physical meaning to the abstract continuity postulates which would form part of any postulational foundation for physics. The essence of this meaning might perhaps be summarized as follows: Some physical laws are so general and fundamental as to be independent of the variations introduced by the quanta of *physically* continuous magnitudes, and such laws accordingly must be stated so as to be independent of *all* variations of some sort; and such a consideration of all variations is essentially a consideration of limits and mathematical continuity.

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[8] *Loc. cit.*, p. 336.

The Theory of Continuity of William of Ockham

BY

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OCKHAM realized the value of a careful study and application of the mathematical theory of continuity, even though he possessed no conclusive empirical evidence that it adequately explained the continuity experienced in the world of sense. He also clearly perceived a relation of continuity to infinity, since continuity, as a property of an order which is possible only to a series of terms, must have an infinite number of terms. "A continuous series must have an infinite number of terms."

Ockham could explain Zeno's outcome of the race between Achilles and the tortoise only by introducing the notion of infinite divisibility and by assuming that, if space and time consist of points and instants, the number of them must be infinite. Continuity acquired for Ockham philosophical and mathematical significance, for he conceived space and time as consisting of points and instants and as having the property of continuity in mathematics.

Ockham anticipated some of the present day conceptions of continuity which he discussed in relation to: (a) space in one, two and three dimensions; (b) substance, quantity and quantum; (c) time; (d) motion; (e) sense-data; (f) whether the whole is greater than its part; (g) causes; (h) his replies to ten objections to the theory of continuity.

I. CONTINUITY IN RELATION TO SPACE

Ockham treats of continuity in space in one dimension in answering the question: "Whether a line is composed of points?" (A).

He differentiates between a continuum as seen in a "compact series," and a contiguum, "things juxtaposed." He defines each when he says that "between the first point of this line and any other point there is some intermediate point, or none. If there is no intermediate, there is therefore one immediate to another (as in contiguity). If there is some intermediate, that point is therefore intermediate between the first point and itself (as in continuity); for that is the first of the number of all the points" (A). In association with this discussion, Burns states: "He says a fallacy is involved in the implied argument: and to this he answers that (1) between the first point and any other there is always a third, but that (2) not any point lies between the first and any other (i.e. as I understand him,—there is no second point in the line, but there is a first)" (D). "His treatment of continuity is based upon the idea that there is always a third point between any two in a line, or that there is not a certain number of units in a continuity" (B, p. 144). He believed that there is an infinite number of points in a line which constitute "a 'compact series' when no two terms are consecutive, but between any two there there are others" (B). "In a 'compact series' the number of terms between any two given terms must be infinite" (B, p. 145). Ockham dissents from Aristotle who makes these statements: "nothing is intermediate between parts of a line and they make one per se," "parts of a line are joined at one common terminus," "those touch each other between which nothing is intermediate" (C, pp. 75, 79), as when contiguous.

Ockham answers the above question when he replies to this question: "Whether any parts of a line are divisible?" (A). He states that "no part of a line is indivisible, nor is even any part of any continuum indivisible" (A). He believed that a line is continuous and that each part is continuous.

Ockham states that although it seems "useless to prove so diffusedly that a continuum is not composed of indivisibles (un-

less there are some such indivisibles), it must be stated that it is not useless to prove one conclusion through diverse intermediates, although it might be proved sufficiently through one intermediate. Whence, an intermediate was sufficient to prove that there is no indivisible; but, however, others would not be superfluous, and this is especially true in this case; for to prove that nothing is indivisible in these inferiors can not be accomplished except through subtle reasons, which mathematicians and others less skilled in metaphysics and logic can not comprehend. But to prove that a continuum is not composed of indivisibles through other intermediates, even if there were such indivisibles, can be accomplished through reasons more apparent to mathematicians . . . and, therefore, the reasons are not superfluous" (C, p. 81). Ockham's discussion seems to provide a basis for the 'compact series" of Bertrand Russell who says that "a series is called 'compact' when no two terms are consecutive, but between any two there are others" (B).

Ockham supports his statement that "no part of a line is indivisible, nor is even any part of any continuum indivisible" (A) by saying that "if any part were indivisible," "then the side of a square would be equal to a diagonal, and so there would be a diagonal commensurable with its side" (A) (D, p. 507), and also by saying that "then there would be an equal number of points in a smaller circle and a larger' (A).

2. In the De Sacramento Altaris, Ockham extends his analysis of continuity from one to two and three dimensional space. His argument relative to a continuous surface and to a continuous body is similar to that showing the relation of a point to a line. He raises the question, "Whether a line and a surface are really distinguished from each other and from a body?" (C, p. 83). He says that "as is the relation of a point to a line so is the relation of a line to a surface, and a surface to a body, but a point is not really distinct from a line; . . . therefore a line is not distinct from a surface nor a surface from a body, and consequently a line and a surface are not distinguished from each other or from a body" (C, p. 83). In his discussion he speaks of "infinite points," "infinite lines," "infinite surfaces," "intermediate points," "inter-

mediate lines," "intermediate surfaces," "continuous surface," and "continuous body." As there are intermediate points between any two points of a line which are recognized as discrete, so there are intermediate lines between any two lines of a surface which are recognized as discrete (a surface is composed of "infinite lines; for there are not just so many so that there could not be more"), and so there are also intermediate surfaces between any two surfaces of a body which are recognized as discrete.

II. CONTINUITY IN RELATION TO SUBSTANCE, QUANTITY, AND QUANTUM

1. Ockham extends his analysis of continuity to quantity and substance when he answers the question: Whether a body which is quantity, is an absolute thing really distinct from substance?" (C, Qu. 3, p. 92).

He states that "a substance has of itself parts; therefore, it is of itself a quantum; therefore, it is not a quantum through another thing" (C, p. 93), that "nothing other could be quantity, concerning which there is a question, than a continuous body per se one, or a thing having parts or a part outside of a part existing circumscriptively in a place" (C, p. 95), and that "every permanent and continuous quantity is a line, or a surface, or a body" (C, Qu. 3).

2. Speaking of "continuous quantum," he says that "a situal order of parts alone suffices for this, that something may be a quantum. But such an order of parts requires local separation of parts, and consequently nothing is a quantum continuous or permanent, unless it has parts separate in situation" (C, p. 341). "A continuous or permanent quantum" is convertible with "having part separate in situation from part" (C, pp. 153, 341).

"Quantity is not a thing really distinct from substance and quality, but any quantity is really the same as substance, and any quantity is really the same as quality." But "every finite quantum continuous and permanent has form, but an order of parts in a place is required for form, if it be present in a place" (C, p. 341). "And so it is manifestly obvious that a situal order of parts is required for form" (C, p. 343), which is a characteristic

of a continuous and permanent quantum. Bertrand Russell says that "continuity, in mathematics, is a property only possible to a series of terms, i.e. to terms arranged in an order, so that we can say of any two that one comes before the other. . . . Continuity is essentially a property of an order; it does not belong to a set of terms in themselves, but only to a set in a certain order. . . . Thus the essence of continuity must not be sought in the nature of the set of terms, but in the nature of their arrangement in a series" (B).

3. Ockham extends his analysis of continuity to condensation and rarification of a quantity. He says that "when a corporeal substance is condensed, I ask whether any part or no part of a quantity is lost. If no part, (and then condensation can be retained), then for the same reason (although substance may be quantity and no part of substance may be lost), condensation will have been retained" (C, p. 129). He says that Aristotle's statement that "that is dense whose parts lie close together" (C, p. 131) . . . "is not to be understood of the parts between which there is not an intermediate; for those can not, as regards themselves as wholes, lie closer together; but it must be understood of the parts between which there are intermediate parts; for those parts lie closer together in a dense body than they did in the same body when it was rare. And for the reason that all such parts lie closer together (whether they are under another thing or not), they in themselves lie closer together and that body is more dense now than before. Whence, I do not see that condensation and rarification is fittingly retained, except by positing that quantity is not a thing other than substance and quality, and through that mode is easily retained" (C, p. 131).

III. CONTINUITY IN RELATION TO TIME AND MOTION

Ockham extends his analysis of continuity to time and motion. To him "time is not some absolute accident really distinct from motion and from permanent things existing subjectively in a whole motion and in no part" (C, p. 19); for "time is in the whole movement of the whole heaven, and yet it is not in any part of the movement of any part of heaven" . . . (C, p. 19). "An instant

is not such an absolute thing really distinct from every divisible thing" (C, p. 51). He regards the following expressions as figurative: "An instant is indivisible" (C, p. 53), "generation is sudden," (C, p. 53), "substantial form is a terminus of generation" (C, p. 53), "to have been changed is the terminus of motion" (C, p. 53). "Generation is in an instant" (C, p. 55) is equivalent to "when something is generated, a part is not generated before a part but the whole is generated at the same time" (C, p. 55). "Motion is in time" (C, p. 57) is equivalent to "when something is moved, it acquires one part before another, or it acquires one before another without interruption" (C, p. 57). Continuous motion implies that between two discrete points occupied at two discrete instants there are intermediate points occupied at intermediate instants.

In the Quotlibeta, Ockham supports his conception of continuous time and continuous motion when he examines the argument of Zeno. He concedes "that in any instant something is acquired and yet a part of that acquired was acquired before and a part of that before, and so on indefinitely. And so the swift mover in this space passes the tardy mover through infinite instants" (A). He concludes that "he, who denies indivisibles can solve the arguments of Zeno, and in no way can he who posits them" (C).

IV. CONTINUITY IN RELATION TO SENSE-DATA

In the Centiloquium, Ockham extends his analysis of continuity to sense-data and says "that any body through finite whiteness can be intensively white infinitely. This conclusion declared that all parts of any corpus uniformly white through a whole can be to each other in turn non-existent so that each part of that body may be non-existent to each part of the same; therefore, any body through finite whiteness can intensively be infinitely white extensively. . . . For having posited that a primary cause may do this, then infinite whiteness can be at the same time equally infinite; therefore, the resulting whiteness would be infinite intensively; therefore, it would make one subject infinite intensively" (E).

Ockham also concludes that "something can be infinitely white

intensively; therefore, there can be something white which can not be seen by any vision of finite virtue. . . . Again, through experience it appears that not only the non-extended may be insensible, but even something extended can be so small that it can not be seen, not even touched, nor perceived by any human sense" (E). Since "a lesser power of whiteness by force of virtue can cause its species to be in an intermediate; therefore, much more strongly can whiteness by force of greater virtue. Again, in the highest there is a disposition to receive a species of any color whatever, therefore, of this infinite color. Again, to-day the same whiteness can in another place cause its species to be more intensively in an intermediate" (E). Ockham anticipates James Ward when he is sure that there must be intermediate shades of color between two discrete shades; for he asserts in the Quotlibeta: "I say that the visions of whiteness continuously succeed one another as the shape is continuously increased. And so, as it is not necessary because of the motion due to augmentation to posit such indivisibles, so it is not necessary either because of visions to posit such indivisibles" (A). C. Delisle Burns states that "this is the reply to Bergson's objection against the 'cinematograph' idea of vision, and it indicates the distinction between the cinema-pictures and actual human vision. For there are not a certain number of visions of white or of shape perceived in change" (D).

V. CONTINUITY IN RELATION TO THE WHOLE AS GREATER THAN ITS PART

In the Quotlibeta, Ockham states "that this proposition, 'the whole has parts more than a part', can be understood in a twofold way. In one way, that a part may have some certain number of parts, and that the parts of the whole may exceed this in any certain multitude; and that judgment is false, for it implies that the whole and a part may have some certain number, which is false. In the other way, that a part of a whole may have parts not so many in some certain number but that it may have more, because infinites, and that the whole may have so many parts and in addition some others. And so I concede that the whole heaven may have more parts than a part" (A).

In the Centiloquium, Ockham says that "it is not unbefitting that a part be equal to its whole or not be less; for this is found, not only, as it were, extensively but even intensively, as appears in the proposition whenever another part is of infinite virtue intensively. . . . This same, however, is found in a discrete quantity or at least in some multitude in some part of which there are fewer unities than in the whole multitude, because in the whole universe there are not more parts than in one bean, because there are infinite parts in a bean. But that principle, 'every whole is greater than its part', ought to be so perceived that every whole extensive, composed of finite integral parts, is greater than any integral part of it, and even that every whole of virtue, composed of finite virtues, is virtually greater than its finite partial part" (E).

VI. CONTINUITY IN RELATION TO CAUSE

In the Centiloquium, Ockham extends his analysis of continuity to the theory of "infinite regress" of causes. He develops the concept of the "total cause" accepted by the Nominalists, which is considered "the aggregate of all the antecedents which suffice to bring about the event" (E).

He disagrees with Aristotle who states that "the causes of things are neither an infinite series nor infinitely various in kind" (E) and "that everything, which is moved, is moved by another and that there is not a process infinitely in causes" (E). Ockham says that "it is possible to urge the other proposition assumed; namely, that there is not a process infinitely in moving causes, since it is clear that there is need necessarily to posit in some moving causes a process infinitely, as it appears, if some long continuum be struck. On account of the striking of one part; namely, the end, the part struck moves a part continuous with itself and that part moves another, and so on infinitely; and according to that there are there infinite parts, at least proportionate according to longitude, of which each is moved by reason of such a percussion; and therefore, it is not impossible but rather necessary in moving causes to posit a process infinitely" (E). Ockham says that Aristotle "deduces that infinite motions would be in finite time, if there could be a process infinitely in moving

causes. It is said that to some it does not seem inconsistent that such a motion of infinite movers ought to be called infinite" (E).

Ockham further states that it is argued that if there is not a process infinitely in moving causes, "it appears from this that, if there is a process infinitely, there would be no intermediate cause; for where there is not an ultimate, there is not there an intermediate, and this demonstration seems to be from a posterior, since it has an origin from an effect, namely, a motion. But although this reason may be more probable to some, yet it does not appear demonstrated" (E). Ockham believed that every effect is the result not of one but of many causes, and therefore he advocates the theory of "infinite regress" of causes.

VII. OCKHAM'S REPLIES TO OBJECTIONS TO THE THEORY OF CONTINUITY

In the Quotlibeta, Ockham replies to ten objections to the theory of continuity.

1. "I say that there is proof that there are indivisibles and that it is not impossible for indivisibles to be, since the intellectual mind is indivisible. But it involves the contradiction, that there is an indivisible in quantum, since neither can there be a part of a quantum nor an accident of it. That there may not indeed be a part has already been demonstrated. . . . And indeed I say that God can not make such an indivisible" (A). "I am not referring to what is not quantative" (D). 2. "That it is impossible and that it involves a contradiction that a sphere can tangentially touch a plane. . . . Whatever part touching tangentially is taken, yet one intermediate does not touch an immediate, nor an intermediate of that intermediate; and so on infinitely" (A). "It is said that God could make a sphere commensurable with a plane tangential to it, Ockham replies that he could not" (D).

3. "I say that this is the difference between a continuum, ('compact series'), and a contiguum, ('things juxtaposed'); since parts of a continuum make one per se, parts of a contiguum do not" (A). "We must distinguish the continuum, ('compact series'), from 'things juxtaposed' " (D).

4. The previously cited argument relative to whether a whole is greater than its part (A). "It is asked whether the whole is

greater than its part and Ockham replies: 'That depends on what you mean by greater. For if you mean that the whole contains a certain definite number of parts more than the part, it is not true. But if you mean that both part and whole may be infinitely divided and yet may be commensurable, then the whole is (in infinite divisibility) greater than the part' " (D).

5. "I say that if the world would have been from eternity, and God would have in every hour made one division in one continuum, there would not be as yet a complete division. And when you say that there would have been more divisions than instants of past time, I speak, as before, of this plurality" (A). "It is said that according to Ockham if God had been dividing from the beginning of the world, he would still be at it. Ockham admits the idea and refuses to view it as absurd (non esset adhuc completa divisio)" (D).

6. "I say we are informed (as far as we are able in this state) through a divisible minimum according to nature; sometimes according to the conventional, as an ell" (A), a standard. "A sixth objection refers to conventional lengths such as a yard, which is obviously frivolous" (D).

7. "Concerning the Blessed Virgin, I say that it could have been impossible that she could have been placed under original sin, even through one instant" (A). "A seventh refers to the Immaculate Conception of the Virgin Mary, which to modern ears may have little indeed to do with infinite divisibility. But Ockham asserts that she never was in Original Sin in any part of an instant" (D).

8. "I say that God can not make two angels passing suddenly (ad saltum) from place to place; but there is need of an intermediate time" (A). "To an eighth implied objection he replies that God could not make an angel pass from one point to another without passing through infinite points between" (D).

9. "I say that visions of whiteness continuously succeed each other, as form is continuously increased. And so, as on account of the motion due to augmentation it is not necessary to posit such indivisibles, so neither on account of visions" (A). " 'As the shape of an object is continuously increased (? by coming near and nearer), so the visions of white succeed one another

continuously, and as we need not suppose indivisible units of change, so we need not suppose indivisible units of vision'. That is to say, as I understand him, there is always a sense-datum perceivable between (in time) the first perceived and any other. This is (1) the reply to Bergson's objection against the 'cenematograph' idea of vision, and (2) it gives the distinction between the cinema-pictures and actual human vision. For there are not a certain number of visions of white or of shapes perceived in change" (D).

10. "This must be conceded, that between the first point and any other there is some intermediate point; but it must be denied, that any point is intermediate between a first and any other" (A).

Burns also asserts that Ockham "seems to have had in his philosophy all the prestige of tradition and all the promise of a new beginning." Burns adds that the work of Ockham "on Continuity and Infinite Divisibility" is "historically interesting, in view of the fact that it was done in the early fourteenth century and there are interesting parallels to be found between the ideas of Ockham and Mr. Bertrand Russell: sometimes there is even a close likeness between the actual sentences in the ninth question in the first book of Ockham's Quotlibeta and those in Mr. Russell's Our knowledge of the External World" (D). It is rather strange that the texts of Ockham, which treat of so many recently discussed problems, have been ignored; for a reading of them would prove most informative, suggestive, and helpful. A more careful study of the ninth Question of the First Book of Ockham's Quotlibeta, of portions of his Centiloquium Theologicum, of the entire text of his De Sacramento Altaris, and his Logica will prove most useful to those who are interested in the theories of continuity and of infinity, in the use and value of mathematical logic so useful to Leibniz, the method of "logical atomism", the method of extensive abstraction, and the new quantum theory, as they are applied in science today and especially in the field of the new Physics.

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Renegade Instances

BY

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ATTENTION has been drawn, particularly since Kant, to propositions which can not have negative instances. They used to be called *a priori*, axioms, first principles. Today, they are usually called postulates—C. I. Lewis uses both the old and new terminology—because there is a growing recognition of the fact that at least some of them are not "necessary" in the traditional sense. Kant placed a limitation on the apriorism of the continental rationalists. Current epistemologists and logicians have outstripped Kant in the direction signalled by him. Both he and they, however, concur in the opinion that apriori propositions are subject to negation *of a sort*. The Kantian *dinge-an-sich*, though they can not contradict these propositions by being negative instances of them, do nevertheless stand to them in the relation of insubordination, thereby exerting on them a "negative action" of a sort. And, according to current views, there are instances which, though they fall outside the range of significance of a proposition or system and hence can not contradict it, do nevertheless tend to bring about a revision of it by a negative action of some sort. The purpose of this brief essay is to coin a convenient name for such instances and to describe certain properties of their peculiar negative action.

We shall best realize this purpose by beginning with the distinction between apriori and aposteriori generalizations. (Strictly speaking, there are no *generalizations* which are simply apriori, as Wittgenstein among others has pointed out. But we here use

the term nevertheless to show that we are concerned not with the abstract apriori laws of formal logic but with propositions which seem to say something "necessary" about matters of fact.) An example of an apriori generalization is, "Swans are birds" (Lewis). Qua apriori, it is of the form.

$$(x)\cdot\phi x\cdot\psi x\cdot \supset\cdot\phi x$$

where "ϕ" is analyzed into, say, the predicative elements a and b, and "ψ" into c and . . . n. As thus analyzed, its form is

$$\{x\}\colon (a\cdot b)x\cdot(c \cdots n)x\cdot \supset\cdot(a\cdot b)x.$$

Substituting "feathered" for a, "biped" for b, "white" for c, "graceful" for d, and so on to n, the result reads: "If anything is feathered and biped and also white and graceful, etc., then it is feathered and biped." Now let the complex of characters $a.b.c$ . . . n be the definition or "intension" of "swan" and $a.b$ of "bird." Obviously, it is impossible for the whole implicative proposition to have any negative instances. There can not be any swans which are not birds. Suppose we are confronted with a feathered, white, graceful, etc. creature, but one having four little fur-bearing legs. Then, by the definition $a.b$ of "bird" and $a.b.c\ldots n$ of "swan," it would be neither bird nor swan, *certainly* not a swan which is not a bird.

Such propositions as the above are analytic, hence apriori or necessary. The implicans consists of a set of elements (intension) which includes all the elements in the set comprising the implicate. Before examining how a proposition of this kind is "infirmed" (Nicod) by our supposititious swan-like quadruped, *without being contradicted by it*, let us consider the form of "Swans are birds" where its analysis results in an aposteriori generalization—one capable of having negative instances any one of which destroys it by the negative action of contradiction.

"Swans are birds," as an empirically determined proposition, is also implicative in form:

$$(x)\cdot\phi x \supset \psi x.$$

But this may be sometimes false, or is not a law of logic. It is "probabilified" (H. H. Price) by the conjunctive proposition

$$(\exists x)\cdot\phi x\cdot\psi x,$$

where "ϕ" now is analyzed into *a.c. . . n* and "ψ" into $b\cdot n + 1\ldots$ these letters having the same empirical meanings we assigned them above. Thus the whole conjunctive proposition

$$\{\exists x\}\colon (a\cdot c\cdots n)x\cdot(b\cdot n + 1\cdots)x$$

probabilifies or inductively yields the hypothesis

$$\{x\}\colon (a\cdot c\cdots n)x\cdot \supset\cdot(b\cdot n + 1\cdots)x.$$

In words, "Something's being feathered, white, graceful, etc. and also biped, etc., probabilifies the hypothesis that if anything is feathered, white, graceful, etc., then it is biped, etc." *Now* our freakish quadruped would falsify the function (b.n + 1 . . .)x, where b means biped, and would therefore be a case of

$$\{\exists x\}\colon (a\cdot c\cdots n)x\cdot \sim (b\cdot n + 1\cdots)x.$$

This strictly implies

$$\sim[\{x\}\colon (a\cdot c\cdots n)x\cdot \supset\cdot(b\cdot n + 1\cdots)x],$$

namely, the falsity of the generalization "Swans are birds," where the intension of "swan" here is the complex *a.c . . . n*, and of "bird" b.n + 1. . . . It is noteworthy that the first formal conjunction, both of whose component functions are true, merely probabilifies our generalization; whereas the second conjunction, containing a false component, *necessitates* the falsity of the generalization.

Such generalizations are synthetic, hence at the mercy of negative instances. The implicate of the hypothesis consists of a set of elements containing at least one element not a member of the implicans-set. Thus the truth of such propositions is not "intensionally determined," and the negative action of unfavorable instances on them is contradiction.

So far, we have been covering fairly familiar territory. We

shall now attempt to describe the peculiar sort of negative action our freakish creature has on the apriori generalization

$$\{x\}\colon (a\cdot b)x\cdot(c\cdots n)x\cdot \supset \cdot(a\cdot b)x.$$

We have seen that it can not contradict it or *logically* negate it, since, in respect of form, the proposition is tautologous. By the law of contradiction, we know that a case of

$$\{\exists\chi\}\colon \phi x\cdot\psi x\colon \sim \phi x,$$

being the contradictory of the tautology

$$(x)\cdot\phi x\cdot\psi x\cdot \supset \cdot\phi x,$$

is impossible. But no one doubts that in some manner our queer feathered quadruped would threaten the generalization, at least in the sense of initiating a process whose result is the revision of the original apriori generalization, or its replacement or supplementation by another.[1] For example, our queer creature might negate the original hypothesis by resulting in the deletion of *b* (biped), yielding

$$\{x\}\colon (a\cdot c\cdots n)x\cdot \supset \cdot(a)x.$$

This, in common speech, is what would happen if it were agreed that "biped" is not an "essential" or defining characteristic of a bird. And, in this event, our feathered quadruped would still be a bird thanks to its feathers, and even a swan thanks to its color, grace, etc.

If the original generalization escaped modification by this instance, the latter would require a revision of some other apriori generalization, as for example the one concerning animals. The definition of animal would have to be revised to allow for a winged, feathered, egg-laying creature's being an animal, and so forth.

[1] More precisely, the feathered quadruped does not disturb the proposition, "If anything is feathered and biped and white and graceful, etc., it is feathered and biped," but it shakes, rather, the variable (see below) "Swans are birds," *with this analytic interpretation*. As we shall see, it exposes the word-variables "swan" and "bird" to redefinition, the new resultant being either another analytic proposition or a synthetic one. This qualification should be kept in mind as we proceed.

Thus, either directly or indirectly, such "queer" instances modify, by deletion or addition of elements, the defining intensions and the "necessary" generalizations in which they occur.

Summarily, then, there may be instances which modify definitions and the analytic propositions which involve them, not by standing to them in the relation of "negative instance of," but in the relation of what I shall call "renegade instance of." And the negative action of such instances on apriori generalizations is not logical or simple negation, but what I shall call "renegation." If this terminology at first appears more picturesque than pointed, let the reader reflect on its Latin ancestry. Furthermore, let him consider what, historically, a renegade is. A renegade is something toward which a system assumes a repellent or expellent front, something which militates against the system, not as a decent negative instance before which an aposteriori system is properly submissive, but as something foreign and external to *necessary* generalizations, threatening them from without by anarchic (non-logical) opposition. Hence "renegade" and "renegation."

We have shown that "Swans are birds" is susceptible of two analyses, according as it is taken as apriori or aposteriori. That raises the pertinent question, which kind is it *intrinsically*? Both Lewis and Carnap ("A rose is a thing") consider it apriori, a "sentence" whose truth is determined by the analysis and statement of the "formal" or "syntactical" meaning of its terms. But both, contrary to Kant, would insist that no generalization is *per se* or intrinsically apriori. Whether it is or is not depends upon the grammatical or logical syntax of the language in use. And this latter may vary from epoch to epoch. At the present stage of modern linguistic usage, "Swans are birds" or "A rose is a thing" normally turns out to be apriori. If they are renegated by a sufficient number of renegade instances—or by a single impressive one—their terms may be redefined, thus transforming them either into different apriori statements or into empirical ones having as negative instances what previously were renegades.

Now I believe it is indisputable that the simple question, "Is our queer quadruped a negative or a renegade instance of the

generalization 'Swans are birds'?" is incomplete and therefore unanswerable as it stands. There is no answer forthcoming until we know whether the analysis of the generalization is to result in an analytic or synthetic proposition. In other words, Carnap and Lewis are right in their denial that such linguistic forms as "Swans are birds" are intrinsically apriori. But we have yet to make a very important distinction. This distinction is between "Swans are birds" *as a variable* capable of having either an apriori or an aposteriori interpretation on the one hand, and these interpretations or values of the variable themselves on the other. Thus, though the variable "Swans are birds" is intrinsically neither analytic nor synthetic, its interpretations are, I submit, intrinsically either one or the other. If its analysis results in a proposition of the form

$$p \cdot q \cdot \supset \cdot p$$

or, converting this into a formal implication,

$$(x) \cdot \phi x \cdot \psi x \cdot \supset \cdot \phi x$$

or, as in our specific instance,

$$\{x\} : (a \cdot b)x \cdot (c \cdots n)x \cdot \supset \cdot (a \cdot b)x$$

then this result is intrinsically apriori, and intrinsically incapable of having negative instances. Similarly, if the analysis of "Swans are birds" results, as it well may, in a synthetic proposition in the manner described above, then this result is intrinsically aposteriori. Any generalization having this form is *necessarily* capable of having negative instances. It is a law of logic that

$$\Diamond[\sim \{(x) \cdot \phi x \supset \psi x)\}]$$

In view of the above considerations, it is advisable to qualify the assertion that the variable "Swans are birds" is an apriori or even an aposteriori proposition, since some analysis of this variable is required to result in that which is analytic or synthetic in form. And we should, furthermore, now supplement our own previous statements about renegatable propositions.

Nothing, not even a renegade instance, can disturb or in any way negate the *formal* principle

$$(x)\cdot\phi x\cdot\psi x\cdot \supset \cdot\phi x,$$

"Anything which is characterized thus and so is characterized thus." This is a law of logic and a tautology. It "says" nothing about empirical matters of fact, hence nothing in that quarter can possibly exert any negative action on it, not even as a renegade instance. But shall we say the same of "Swans are birds" where this is analyzed into the above analytic statement about feathered bipeds, etc. Though it, too, is indeed analytic in form, is it a law of logic, a formal tautology? Is the statement, "If anything is feathered, biped, white, graceful, etc., it is feathered and biped," a purely logical principle? The answer is, of course, that this statement is not itself a formal logical principle, but is, rather, a particular concrete interpretation of one. For "ϕ" and "ψ", adjectives having empirical denotation are substituted. In this respect if in no other, the statement is certainly what Carnap calls an "object" sentence, not a purely syntactical one—though the *form* of the combination of its elements is apriori. And this brings us to the crux of the matter concerning renegade instances. Only those analytic statements some of whose symbolic constituents stand for empirical qualities (or for some of whose symbolic constituents empirical substitutions are made), only such apriori statements are capable of having renegade instances.[2] By virtue of this reference to non-syntactical or extra-systemic elements, the generalization, despite its apriori form, exposes itself to modification by empirical orders or empirically discovered—not postulated—combinations. Let us call such propositions "empirical apriori generalizations." "A rose is a thing," even when analyzed into an analytic "sentence" (Carnap) is, in this light, an object sentence capable of having renegade, though no negative, instances. (Carnap noticed correctly that "A rose is a thing" can not have negative instances qua apriori, but this misled him into the assumption that it is wholly

[2] See first foot-note.

"about words," a sentence whose truth is determinable by syntactical considerations alone. Thus he overlooked the fact that it can be *empirically renegated*, or rendered "inconvenient" by an *object*. Bronstein, in the April 1936 issue of *Analysis*, also shows that apriori sentences have an objective reference in a manner unsuspected by Carnap, though this article as a whole is, I believe, unjust to Carnap's position.)

Purely formal principles, however, such as are empirically uninterpreted laws of logic, can not possibly suffer negation of any sort. Even their formal denial implies their truth. These may be simply ignored, but that is not being negated. Let us call such propositions "formal apriori principles," contrasting them with "empirical apriori generalizations."

So we have discovered two kinds of negating instances, negative and renegade. Negative instances *contradict* generalizations which are aposteriori in form. Renegade instances *renegate* generalizations apriori in form, such analytic or apriori statements being called "generalizations" because they have an empirical reference *via* some of their symbolic constituents. But there are formal apriori principles or sentences or laws or tautologies—call them what we will—which, as empirically uninterpreted, are exempt from negative action of any sort, because they are not *generalizations* at all. Of these, according to purpose, we make certain selections to serve as "first principles," and modern logicians know what an indefinitely large number of such "tautologies" we have to choose from. Whether they be creatures of human postulation or the most abstract structural principles of extra-conceptual existence it is hard to say. But what we have said here is not concerned with that matter and leaves the question open. My guess is that what the conceptualists and positivists have said in defense of the man-dependent theory leaves it rather unfortified.

Two minor notices in closing: why are some apriori empirical generalizations ("Swans are birds") rejected, and why some formal apriori principles? Are the reasons for the rejection similar in both cases? I think not. The former are rejected when they fail, in the peculiar manner suggested above, to *represent* or stand

for something—are renegated by something—and the latter when they fail to *imply* something, i.e., when they can not be used in the formal deduction of some desired consequent. Perhaps this difference serves further to distinguish these two apriori types. As to the second and last notice: since there are two kinds of infirmation, namely, negation and renegation, there are two corresponding kinds of *confirming* instances, positive and —? How shall we name the opposite of "renegade" and "renegation"? Positive instances probabilify aposteriori generalizations. What confirms an empirical apriori generalization? Perhaps we could call them "corroborative" instances and distinguish them from the "positive". But naming and describing them, together with their corroborative action on a "necessary" generalization, is not within the scope of this essay.[3]

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[3] The reader who is versed in the history of science may replace our fictitious renegade instance (the feathered quadruped) by actual historical renegades—instances which rendered "useless" or "inconvenient," by renegation, the basic assumptions or "first principles" of scientific theories.

Pseudo-Scientific Economic Doctrine—*Continued*

BY
JOSEPH MAYER

PART II. SUPPOSITIONS UNDERLYING COST AND UTILITY THEORY

THE analyses thus far undertaken of cost theory and utility theory have viewed these doctrines as they originally were, namely, as the contentions of two conflicting groups of value theorists. And had the two rival groups been allowed to continue to match strength on the plane on which Macvane, for example, contended, the result would probably have been the destruction of them both. Underneath surface conflicts, however, the rival theories had much in common. As the twentieth century got under way, this state of affairs was discerned, with the result that in the end there was hand-shaking and backslapping all around and the inauguration of a live-and-let-live policy. Although at first regarding each other as mortal enemies, these two groups of price-determination theorists later came to see in one another something resembling blood kinship. But are the underlying assumptions which classicism and neoclassicism have in common any more realistic and scientific than the specific assumptions already examined? This is a question to which we now turn.

In the adoption of a live-and-let-live policy by the rival theorists, cost became "disutility" and its advocates became satisfied with arguing that disutility and utility are simply two

sides of the same shield (Seligman), two blades of the price-determination scissors (Marshall), or equivalent elements in systems of mechanical and organismic concords. This rapprochement meant the retention of much of the earlier classical dialectic and its further refinement.[52]

But if utility and disutility are related, then some of our criticisms of cost probably apply also to utility: first, regarding obstructions to free competition; second, regarding lack of a common, homogeneous unit; third, regarding normalistic, hedonistic, and rationalistic assumptions.

Regarding the *first* of these considerations, that is, obstructions to free competition from the points of views of demand and supply, it was soon apparent that the same reasoning Böhm-Bawerk used to show deficiencies in cost theory could be turned against his own hypotheses, as Macfarlane indicated, for there are obstructions to competition among consumers, as well as among producers.[53]

The *second* consideration, or the lack of homogeneity and commensurability in utility and disutility, was searchingly analyzed by Persons on the score of inequality of incomes. Persons objected to Marshall's statement that economic events "affect in about equal proportions all the different classes of society," pointed to the glaring inequalities of wealth in Great Britain (13 per cent of the population in 1904 receiving one-half the total income), and indicated that existing value relations would be materially changed if wealth were more equitably distributed. Persons then showed that the "last units" of utility for the non-competing groups of the millionaire, well-to-do, and poor classes are non-comparable. In a depression, for example, the rich lose luxuries, the well-to-do lose conveniences, and the poor lose

[52] E. R. A. Seligman, "Discussion," *American Economic Association Publications*, 3rd Series, Vol. II, February, 1901, pp. 247–249; also his "Social Elements in the Theory of Value," *Quarterly Journal of Economics*, May, 1901, pp. 321–347; and Alfred Marshall, "Principles of Economics," 4th ed., *Macmillan*, London, 1898, Vol. I, p. 428.

[53] *Cf.*, Eugen von Böhm-Bawerk, "Positive Theory of Capital," *Macmillan*, London, 1891, pp. 199–233; C. W. Macfarlane, "Value and Distribution," *Lippincott*, Philadelphia, 1899, pp. 42–53.

necessities. Such losses are irreconcilable with respect to "units" of utility, both in "kind and quality."[54]

Disutilities show similar discrepancies on the score of pains suffered. The "final units" of disutility for ditchdigger, well-to-do farmer, and surfeited millionaire cannot be compared, averaged, or reduced to a common denominator. And any attempt to add the sacrifices of capitalist and landlord to those of laborer renders the situation still more incongruous.

In general, considering the valuation of both goods and services, Persons concluded that no common homogeneous unit of utility or disutility can be posited, unless one assumes an equal distribution of wealth, productive capacity, and services, among laborers, landholders, and capitalists. . . .[55]

If we add to these criticisms, based primarily upon inequalities of income, those already examined, that is, difficulties of equating labor-pains to elapsed times, of comparing the unique desires and appetites of different people, and of limiting the concepts utility and disutility to production and consumption respectively (rather than viewing them in terms of human welfare),—there seems altogether no real basis, with respect to price determination or otherwise, for hypothesizing common units among the heterogeneous psychological states involved in the ambiguous concepts of "utility" and "disutility."

[54] *Cf.*, C. E. Persons, "Marginal Utility and Marginal Disutility as Ultimate Standards of Value," *Quartery Journal of Economics*, August, 1913, pp. 547–578; and Marshall, *op. cit.*, Vol. I, pp. 205–206. *Cf.*, also, J. B. Clark, "Distribution of Wealth," *Macmillan*, New York, 1899, p. 380, who says:

"Similarly, if all society acts in reality as one man, it makes such utility measurements of all commodities, and the trouble arising from the fact that there are many measures disappears. A market secures this result, for society acts as a unit—like an individual buyer." For a later discussion, admitting the effect of inequality, *cf.*, F. W. Taussig, "Principles of Economics," 3rd ed. rev., Vol. I, pp. 116–133, especially pp. 124–129.

For the distribution of British incomes in 1904, see Taussig, *op. cit.*, Vol. II, p. 255. It is interesting to note that the distribution at that time in Great Britain was about the same as the distribution of incomes in the United States in 1929. *Cf.*, Leven, Moulton, and Warburton, "America's Capacity to Consume," *Brookings Institution*, Washington, D. C., 1934, p. 54, where it is indicated that 13 per cent of American families in 1929 received around 49 per cent of the income.

[55] Persons, *op. cit.*, pp. 560–576. *Cf.*, also, Clark, *op. cit.*, pp. 391–392.

The *third* consideration, having to do with normalistic, hedonistic, and rationalistic assumptions, is broader than the other two and in part includes them. It is well illustrated in the refinements of thought that followed cost-theory and utility-theory rapprochements, of which J. B. Clark's system is the most noteworthy example and Thorstein Veblen's criticism the most penetrating.

Veblen's Criticism

Veblen recognized Clark as one of the great figures in economics and his system as a direct continuation of classical thought, including both cost and utility arguments on a plane of equality. Veblen also recognized that Clark's system, like other classical systems, emphasizes distribution factors primarily. Though space is devoted in such systems to consumption and production, these are construed almost entirely in acquisitive pecuniary terms and "all human motives tend to be reduced" to such terms.[56]

Classical notions of "primitive life," Veblen continued, simply throw the normalized competitive suppositions into the foreground, and make "natural" and "normal" coincide with the conventional, which, in turn, is accepted as "just and equitable" and as inevitable under the hypothetical freedom of competition. Any departures from this "natural state of man" are regarded as temporary "aberrations," "excesses," or "perversions," which if let alone will right themselves. As a corollary of these suppositions, Veblen went on, the use of "statics" and "dynamics" is purely perfunctory, and under Clark's system these terms are virtually interchangeable. The more "dynamic" the situation, the nearer do we come to a "static" ideal, which is the natural "norm of competitive life."[57]

[56] *Cf.*, John Bates Clark, "Essentials of Economic Theory," *Macmillan*, New York, 1907; and Thorstein Veblen, "Professor Clark's Economics," *The Quarterly Journal of Economics*, February, 1908, pp. 147–195 (148–150).

[57] Veblen, *op. cit.*, pp. 151, 154–157.

J. B. Clark, *op. cit.*, pp. 196–197. Of J. B. Clark's other works, see also: "The Philosophy of Wealth," *Ginn*, Boston, 1894; "The Distribution of Wealth," *Macmillan* New York, 1899; and "The Control of Trusts," *Macmillan*, New York, 1901.

Furthermore, static normality, Veblen pointed out, is merely another name for hedonistic equilibration, cumulative evolutionary changes being left entirely out of account. Whether among the superstitious Aleutian fishermen or among modern factory workers, economic processes are in classical theory presumed to be exercises in hedonistic computation, all substantially alike except for arithmetical details. Questions of "genesis, growth, variation, process," are nowhere provided for.

The effect of such ideas on the meaning of capital is to force the omission of its most essential, intangible, and pecuniary features. It becomes wholly mechanized and materialized, so that, for example, the process of grinding the rough edges off plowshares before they leave the factory might properly be regarded as removing some of the "capital" embodied in them.[58]

Another important effect of such ideas is the inference that capital and labor each has a specific productivity and that, under the "ideal competition" of the "static state," each gets the exact amount of wealth it creates, that is, a reward proportionate to its contribution. The difficulty here lies, of course, with the assumption of mechanical and interchangeable units (whether of labor, capital, disutility or utility). These units are not homogeneous and interchangeable, while, on the other hand, the product is inseparably joint. The specific productivity contention really amounts to saying that the worker gets what he can and the consumer pays what he has to, which is true enough but hardly fruitful with respect to economic theory.

Elaborating upon the difficulties inherent in the dogma of reward according to specific productivity, Veblen found two other suppositions in Clark's system which are inconsistent with it. One is the assumption that all but a few marginal individuals secure a consumer's surplus of utility. How can this be, Veblen asked, if according to "the natural law of final productivity," each man gets only the precise hedonistic *equivalent* of "the total product that he personally creates?" The other supposition has to do with monopolistic gains. These, it should be noted, follow consistently from the pursuit of that phase of hedo-

[58] Veblen, *op. cit.*, pp. 159–166.

nistic logic which aims at maximizing utility and minimizing cost, at limiting supply at that critical point where price multiplied by quantity will be the greatest possible. But such logic also contradicts the specific-productivity argument, for at the critical price the monopolist not only gets the full product of his labor but a surplus, as much surplus in fact as he could possibly get. And, by classical assumption, the consumers also get a maximum of "effective utility" at the monopolistic price. Thus both sides seem to get the classical maximum; and still monopoly gains are spoken of by utility theorists as "robbery" and "plunder"!

The inconsistencies Veblen thus disclosed lie at the very heart of classical and neo-classical theory, which in part turns upon the contradictory presumptions of a balance between production and consumption on the one hand and of an outstripping of consumption by production on the other. One premise leads to "specific productivity"; the other to "consumer's surplus" and "monopoly gains"; both of which diametrically opposed presumptions the classicists and neo-classicists appear to embrace with equal facility.

Behind all these normalistic-hedonistic-rationalistic dogmas, Veblen continued, lies the idea of "one right and beautiful" economic mechanism, which is now here, which is inevitable, and which we can do nothing about, except possibly to keep the wheels from getting off the track.[59]

In a later analysis, Veblen went over utility-disutility arguments more generally, re-emphasized the foregoing difficulties, and laid particular stress upon the failure of classicism to deal with cumulative institutional forces and with pecuniary factors.

Modern research, he reiterated, has indicated that economics

[59] *Ibid.*, pp. 168–182, 190–195. For an excellent analysis and refutation of Clark's specific productivity doctrine as such, see W. M. Adriance's "Specific Productivity," *Quarterly Journal of Economics*, November, 1914, pp. 149–176. *Cf.*, also, Joseph Mayer, "Consumer's Surplus," *American Economic Review*, March, 1926, pp. 77–80; P. G. Wright, "Total Utility and Consumer's Surplus," *Quarterly Journal of Economics*, February 1917, pp. 307 ff; H. E. Miller, "Utility Curves, Total Utility, and Consumer's Surplus," *Quarterly Journal of Economics*, February, 1927, pp. 292 ff.; R. S. Meriam, "Supply Curves and Maximum Satisfaction," *Quarterly Journal of Economics*, February, 1928, pp. 169 ff.; G. W. Terbough, "Psychic Income," *American Economic Review*, March, 1928, pp. 75 ff.

must analyze the "life-history of material civilization," which is primarily "a cumulative sequence of habituation," and which varies incontinently, cumulatively, and withal somewhat consistently. This institutional process of habit and custom is completely omitted by utility-disutility analysis or is treated as inconsequential.

With respect to pecuniary institutions, the unreal classical point of view has had the effect of a rather peculiar inversion of ideas. Price conventions not only dominate and permeate all modern business activity, but they are also reflected in behavior which is essentially non-pecuniary; and yet hedonism and classicism would have us believe that even in business enterprise it is not questions of price, of pecuniary income and outgo, that are dominant, but, rather, hedonistic computations of utility-disutility. Thus the money phraseology is made to disappear only to come back again, "theoretically expurgated," as a "refined system of barter" between hedonistic magnitudes. Such an inversion of pecuniary logic has been woven into the very tissue of classical and neo-classical theory. It is small wonder that marginal dogmas which foster this anomaly should have missed the chief elements of our economic life.[60]

Mitchell's and Downey's Analyses

Following Veblen's penetrating attack upon the premises underlying classical economic theory, several important elaborations and extensions of his criticism appeared, among which the analyses of Mitchell are outstanding. After pointing out the false psychological premises underlying classical economics, Mitchell reviewed four types of subsequent developments in theory.[61]

[60] Veblen, "The Limitations of Marginal Utility," *Journal of Political Economy*, November, 1909, pp. 620–636. Of Veblen's other works, see also: "The Place of Science in Modern Civilization," *Huebsch*, New York, 1919; "The Theory of Business Enterprise," *Scribner*, New York, 1920; "The Vested Interests and the State of the Industrial Arts," *Huebsch*, New York, 1920; "The Engineers and the Price System," *Huebsch*, New York, 1921.

[61] *Cf.*, Wesley C. Mitchell, "The Rationality of Economic Activity," I and II, *Journal of Political Economy*, February–March, 1910, pp. 97–113, 197–216 (Part I, p. 103). *Cf.*, also, William McDougall, "An Introduction to Social Psychology," rev. ed., *Luce*, New York, 1926.

The first two types, non-Euclidean and mechanical self-interest theories, may be passed over briefly. The first is represented by Pantaleoni's idea of "an idle science though a true one," the answer to which is that in economics we are interested in actual conditions, not in the unreal premises of an "idle science." The second type, the mechanics of self-interest, is akin to the first, except that the admittedly unreal premises laid down are supposed to be a "simplification" or "schematization" drawn from actual conditions for the purpose of affording a clearer understanding of the facts. The difficulty here is that the assumptions used in the hedonistic calculus are now known to be inapplicable to reality, which, to follow the methods of science, should mean their total rejection and the postulation of experimental hypotheses.[62]

The third type of more recent theory, eclecticism, claims to pick out the best in all preceding theory and to discard the rest. But the best of the classical ideas are still unreal, so that the net result is the retention of the form if not the substance of past errors. Thus eclectics continue to commit "the intellectualist fallacy," which assumes economic behavior to be primarily rational, when as a matter of fact much of it is irrational.

Associated with these unreal assumptions, Mitchell continued, is the tendency, stressed by Veblen, to invert the meaning of pecuniary concepts. Adam Smith set the precedent for this inversion by regarding money as superficial, when in fact the habits engendered by pecuniary concepts have in the course of time come to dominate much more than business transactions, have conserved thought, have heightened industrial efficiency, and have rendered huge coöperative undertakings possible. Mitchell reviewed two particular errors that have resulted from the inversion of pecuniary logic: one, the placing of modern man and the savage on substantially the same footing in economic activity; the other, the substitution of artificial subjective-cost, hedonistic, and marginal-utility concepts for the real money facts

[62] Mitchell, *op. cit.*, pp. 107–108, quoting Maffeo Pantaleoni, "Pure Economics," tr. by T. B. Bruce, *Macmillan*, New York, 1898, pp. 3, 9, 10.

Cf., also, Mayer, "Toward a Science of Society," *op. cit.*, pp. 162–165.

of economic life. The theorist thus remains "superficial where he means to become profound."

How could such an inversion and neglect occur, Mitchell asked, without the situation becoming glaringly apparent? Because, although pecuniary concepts were "read out" of actual practice by the classicists, an analogue of pecuniary logic was "read into" the substituted unreal abstractions and was then by an extension of scope made universal and automatic in all human behavior. The effect of this inversion was not merely formal. Its extension to cover irrational human activity, its exaggeration of rational calculations, its failure to take into account the fact that pecuniary logic is relatively hard for people to comprehend, caused the utter obscuration of really vital economic issues.

In brief, eclecticism continues to take economics out of its evolutionary setting, regards pecuniary concepts as empty symbols, makes for an artificial and superficial view of business phenomena, obscures the relation between pecuniary value and serviceability, and saves itself from utter absurdity only by making its unreal conceptions run roughly parallel to the realities of pecuniary practices.[63]

The fourth type of recent economic theory, Mitchell continued, is the evolutionary, which endeavors to study economics in terms of modern psychology and of institutional development. Actual human nature is here made the heart of the study, pecuniary institutions are investigated as an outgrowth of habit and custom, explanations are couched in "genetic" terms, institutions are traced to habits and habits to instincts, and basic human-nature types of economic conduct are differentiated from those engendered and enforced merely through the power of long-established institutions. . . .[64]

[63] Mitchell, *op. cit.*, Part I, p. 111; Part II, pp. 199, 206–216.

[64] *Ibid.*, Part I, pp. 111–112, Part II, p. 216. Of Mitchell's other writings, see also: "The Backward Art of Spending Money," *American Economic Review*, June, 1912, pp. 269–281; "Human Behavior and Economics: A Survey of Recent Literature," *Quarterly Journal of Economics*, Nov., 1914, pp. 1–47; "The Rôle of Money in Economic Theory," *American Economic Review, Supp.*, March, 1916, pp. 140–161; "Wieser's Theory of Social Economics," *Political Science Quarterly*, March, 1917, pp. 95–118; "Bentham's Felicific Calculus," *Political Science Quarterly*, June, 1918, pp. 161–183; "Making Goods and

Writing at about the same time as Veblen and Mitchell and along similar lines, additional analysis of the questions here under review was made by Downey, who stressed two points; the process of individual valuation, and the matter of price determination.

Regarding the first of these, Downey observed that habit, not reasoned choice, governs most of our acts, that habits are mainly conditioned by existing conventions, that non-habitual acts are motivated as often by outside suggestion as by deliberation, and that marginal utility theory, with its exclusive concern with rationalistic appraisal, thus misses the most important features of the individual valuation process. Nor is anything here gained by the marginalists in substituting "gratification" or "psychic income" for "utility" and in denying hedonistic implications. Deprived of its hedonistic-rationalistic basis, marginalism reduces itself to the unobjectionable although puerile statements that people will buy only what they want, that in a perfect market they will all pay alike for a given commodity, and that they will offer only what they are willing to pay. One might add, also, that what they are *willing* to pay is conditioned by what they are *accustomed* to pay and by what they can *afford* to pay, and that what they *want* is conditioned by what they *think* they want or are *made to believe* they want.[65]

As for the second point, price determination, Downey insisted that marginal utility analysis is likewise futile even in the narrow rationalistic sphere of business activity in which it might conceivably be thought to apply. Nowhere does it get any further than the superficial conclusion, which should be an initial premise, that demand and supply determine price.

Making Money," *Economics and Engineering*, Joint session of the American Economic Association and the American Society of Mechanical Engineers, Dec. 6, 1922; "The Prospects of Economics" in "The Trend of Economics," edited by R. G. Tugwell, *Knopf*, New York, 1924, pp. 3–34.

[65] E. H. Downey, "The Futility of Marginal Utility," *Journal of Political Economy*, April, 1910, pp. 253–268. *Cf.*, also, Mayer, "Comparative Value and Human Behavior," *op. cit.*

Take hog-prices as an example. We start with the demand and the supply of hogs and analyze further. The first thing we then encounter is a complex market organization which can cause wide price fluctuations without a change either in pork-eating habits or in the aggregate supply of hogs. It is these institutional market forces which apparently determine price, and these marginalism fails to touch. Furthermore, these forces appear to determine not only price, but the so-called "margin" as well. This Downey elucidated in an examination of the demand and supply considerations that determine the price of turkeys at Thanksgiving time, reviewing the influence of such factors as ceremonial custom, population and wealth distribution, marketing and transportation facilities, and the accidents of the particular growing season. If the "margin" is itself the resultant of price-fixing forces, he asked, how can it be regarded as determining the price?

Downey concluded, as Veblen and Mitchell had, that institutional settings and customary ways of thought are the important factors for economic analysis. Price is determined by these, not by so-called utility and disutility with their unreal and fallacious assumptions.[66]

The Dogma of Unfailing Mutual Gain in Exchange

As an example of the unreality of modern dialectical refinements of classical and neo-classical theory, we may profitably examine the dogma of unfailing mutual gain in exchange, which examination will also serve to clarify another fallacy associated with such theory, namely, the assumption that, in modern money exchanges, barter transactions still predominate. In general, the dogma of mutual gain maintains that, unless there were a gain in "utility" to both parties, no act of exchange would occur, as in the giving of five dollars for a hat, and that, expunging the money medium in accordance with the classical inversion of pecuniary logic, the transaction reduces itself to a barter between goods and services, or to the exchange of a day's work for the

[66] Downey, *op. cit.*, pp. 263–268.

hat. As a matter of fact, not only does the assumed gain in "utility" continue to be superficially measured by the effaced money, since goods and services are compared in pecuniary terms, but the dogma is distinctly mischievous and false in that the implied barter transactions do not in general occur in modern society.[67]

Regarding the superficial character of the dogma, take the exchange of a horse and a cow as an example which has rendered yeoman service. If we go behind the moment of exchange, considering the utilities enjoyed and the disutilities undergone from the time of original possession, it becomes obvious that in the complete accounting there may just as often be a loss as a gain. But what about the argument when limited, as it usually is, only to the act of exchange itself? Does it continue to have any serious meaning? It will help to a clearer understanding here if the issue is resolved into two parts: first, having to do with the nature of the "gain" in the act of exchange; second, having to do with the nature of the mechanism of exchange itself.

As for the nature of the alleged "gain," it is obvious that the act of exchange is a special aspect of general human behavior. We may therefore say that man "gains utility" in every act or he would remain quiescent. Thus broadened, the doctrine includes both rational or conscious and irrational or subconscious behavior. This is as it should be, and the result is as applicable to economic acts as to others. Since most of our activity is subconscious however, the alleged "gain" in "utility" cannot be for the most part rational in character. And if we wish to assume that the calculations of gain and loss nevertheless go on subconsciously, we must perforce extend the calculus to all organic processes and to all organisms, which leaves us with the conclusion that the imputed "gain" is the "preference" of organic life over death. But such a biologic truism robs "value,"

[67] *Cf.*, Fairchild, Furniss, and Buck, "Elementary Economics," *Macmillan*, New York, 1926, Vol. I, p. 297; also, *idem.*, discussion on pp. 308–310; and Mitchell, "The Rationality of Economic Activity," *Journal of Political Economy*, March, 1910, pp. 205–216.

"gain," and "utility" of all technical economic meaning. From this point of view, then, the dogma is superficial and misleading.[68]

As for the analogy of swapping or barter, it may readily be seen that this does not for the most part fit modern exchange conditions. We do not ordinarily attach utility to each article we produce and then swap for something else. We produce for the market, and the return we receive is a monetary return. Modern man is interested in this return, in this purchasing power (as Macvane realized more than two score years ago), so that, insofar as the dogma of mutual gain rests upon the swapping or barter assumption, it rests upon a false premise.[69]

On any realistic basis, the matter should be stated in terms of the exchange of an individual's income or purchasing power for the things and services that satisfy his wants. For example, to a wage earner the question then becomes, does he feel that his salary is sufficient to cover his wants or that he is being paid what he thinks he is worth? Most people, it may be assumed, do not think that they are being paid more than they are worth; but such an obvious absurdity is precisely what the dogma of mutual gain would have us believe with respect to all exchanges

[68] With respect to the importance of judgment in valuation, see: R. B. Perry, "Economic Value and Moral Value," *Quarterly Journal of Economics*, May, 1916, pp. 452–453, 457.

If it be objected here that the discussion falls outside the field of economics proper, reference is made to Mitchell, "The Rationality of Economic Activity," *op. cit.*, and to his emphasis upon the need in economic theory of reaching down to psychological foundations; also to Frank H. Knight's writings, especially "Economic Psychology and the Value Problem," *Quarterly Journal of Economics*, May, 1925, pp. 372–409, in which the importance of the relation of economic theory to broader philosophical questions is stressed. *Cf.*, Mitchell, "The Rationality of Economic Activity," *op. cit.*, pp. 98–102, 198–200; Downey, "The Futility of Marginal Utility," *op. cit.*, pp. 255–256; Z. C. Dickinson, "Economic Motives," *Harvard Economic Studies*, XXIV, Cambridge, 1922; F. A. C. Perrin, "The Psychology of Motivation," *Psychological Review*, May, 1923, pp. 176–191; E. L. Thorndike, "Educational Psychology," *Columbia University*, New York, 1913, Vol. I; R. G. Tugwell, "Human Nature in Economic Theory," *Journal of Political Economy*, June, 1922, pp. 317–345; Graham Wallas, "The Great Society," *Macmillan*, New York, 1914; Clark Wissler, "Man and Culture," *Crowell*, New York, 1923. For other meanings to preference, see Mayer," Comparative Value and Human Behavior," *op. cit.*

[69] *Cf.*, Veblen, "Limitations of Marginal Utility," *Journal of Political Economy*, November, 1909, p. 635; and S. M. Macvane, "Marginal Utility and Value," *Quarterly Journal of Economics*, April, 1893, pp. 271–273.

of labor for income, for otherwise (it is assumed) these exchanges would not take place!

To sum up the argument, there are, in brief, three specific difficulties in the dogma of mutual gain: (1) that if it is applied only to the immediate act of exchange it is meaningless, whereas if all relevant utilities and disutilities are taken into account the result may just as often be a loss as a gain; (2) that it either presupposes an all-pervasive rationality in human acts, which is contrary to fact, or it reduces man to a subconscious calculating device, which robs "gain" and "utility" of all economic meaning; (3) that it presupposes as a general practice the continued existence of the (for the most part) long-since-departed custom of swapping or barter of goods for goods, whereas modern production is almost entirely for the market and in terms of the purchasing power secured.

The grain of truth which undoubtedly gives the dogma of mutual gain in exchange most of its continued vitality is the fact that the complex modern industrial system has brought an immense gain in "utility" to every one as compared with the benefits of a more primitive economy. But it is mass production under the machine process which is responsible for these benefits, not acts of exchange merely. The medieval notion was that exchanges were in themselves bad, in that one person's gain was considered some one else's inevitable loss. Hence the dogma of mutual gain was developed. But no one any longer questions the virtue of an exchange economy. The dogma has thus long since become an anachronism, and it would doubtless by this time have been buried with honors and have been allowed to rest in peace, were it not still needed to bolster the contentions of normalism, hedonism, and rationalism in economic theory.[70]

Pseudo-Analogies: Mechanistic and Organismic Fallacies

Besides normalistic, hedonistic, and rationalistic assumptions, mechanistic and organismic analogies need to be examined before

[70] J. M. Clark, "The Socializing of Economics," in "The Trend of Economics," edited by R. G. Tugwell, *Knopf*, New York, 1924, p. 82, also his "Social Control of Business," *University of Chicago Press*, Chicago, 1926, pp. 6–7, 37–39, 132–138.

our analysis of the preconceptions underlying cost and utility theory is complete.

Mechanistic analogies are found in the thinking of all peoples and are responsible for a great deal of pseudo-scientific reasoning in social study.[71] At a time of epoch-making advance in any discipline, the dominant type of social theorizing reflects the current vogue. It was not strange, therefore, in the seventeenth century, when mathematics and physics experienced a tremendous impetus, that social study became permeated with mathematical and physical analogies and that the writings of students of society were filled with references to man as a kind of machine or automaton, to individuals and societies as a kind of astronomical system, and to similar analogical references. More recent mechanistic analogies have added little to this seventeenth century product, except possibly in such amplifications as are represented in the works of Bentham and Herbart.[72]

A number of illustrations of such mechanistic analogies have been collected and criticized by Sorokin, who finds them inadequate on two counts: They constitute a two-edged logical fallacy; and they are not borne out by actual experience.

Logically, they either apply narrow judgments (physical ones) to broader phenomena (social) to which they apply only in part,

[71] *Cf.*, Mayer, "Toward a Science of Society," *op. cit.*, pp. 172–173; "The Techniques, Basic Concepts, and Preconceptions of Science and Their Relation to Social Study," *op. cit.*, pp. 458–462.

[72] For further details regarding such borrowings, see Pitirim Sorokin, "Contemporary Sociological Theories," *Harpers*, New York, 1928, pp. 3–37. *Cf.*, also, E. Spektorsky, "The Problems of Social Physics in the Seventeenth Century," in Russian, vol. I, *Imperial Warsaw University*, Warsaw, 1910, pp. 554–558, vol. II, *S. Kul-zhenko*, Kiev, 1917, pp. 420–422; Samuel von Pufendorf, "Elementorum Jurisprudentiae Universalis," *Clarendon Press*, Oxford, 1931, 2 vol. (reproduced from edition of 1672). Regarding Erhard Weigel's views, see Spektorsky, *op. cit.*, pp. 488–563 (545–546). *Cf.*, also, Antonio Portuondo y Barcelo, "Essais de Mécanique Sociale," *M. Giard*, Paris, 1925; V. M. Bechtereff, "Collective Reflexology," in Russian, *Kolos*, Petrograd, 1921, pp. 221–420 (225); H. C. Carey, "Principles of Social Science," *Lippincott*, Philadelphia, 1858–1860, 3 vols., vol. I, pp. 41–43, vol. II, pp. 41–42, vol. III, pp. 466–468, *passim*; also "The Unity of Law," *H. C. Baird*, Philadelphia, 1872, pp. 127–129; Johann F. Herbart, "A Text-Book in Psychology," trans. by M. K. Smith, *D. Appleton*, New York, 1891; A. J. Lŏtka, "Elements of Physical Biology," *Williams & Wilkins*, Baltimore, 1925, pp. 13–17; Mitchell, "Bentham's Felicific Calculus," *op. cit.*; Sorokin, *op. cit.*, pp. 11–23; Léon Winiarski, "La Méthode Mathématique dans la Sociologie et dans l'Economie Politique," *La Revue Socialiste*, vol. XX, 1894, pp. 716–730, and other articles by Winiarksi along similar lines.

or they read into the narrower sphere implications which do not properly belong there. "Social physics" is thus like a hypothetical science of "ten-cent cigars," based upon physical principles which in reality apply to all material phenomena but in the pseudo-science are made to appear as the special characteristics of ten-cent cigars or of society alone. At the same time, in mechanistic analogies applied to society, the physical facts are rendered anthropomorphic.

From the point of view of actual experience, we find no indication that the presumptions of mechanistic social analogies are borne out in practice. There is no evidence that hunger or sex compel attention directly as the mass and inversely as the distance; nor are any other generalizations of this character empirically verifiable.[73]

Economic applications of mechanistic analogies have already been sufficiently illustrated in these pages. Natural normality, hedonistic calculations, an equilibrium of economic forces, statics versus dynamics, are suppositions drawn by analogy from physical science; they were projected when these sciences became the vogue; and they continue to be employed because of the momentum gained in authoritative promulgation. That they lead for the most part to unwarranted and fallacious conclusions should finally in our day become sufficiently clear. . . .

When biologic generalizations came to the fore in the nineteenth century, analogies drawn thereform also penetrated social and economic thinking, especially the belief that society is an organism. Such analogies are likewise not new, but because the more modern nineteenth century organismic view is the most advanced, we shall confine our attention to it here, the contemporary notion being summed up thus: (1) society is a special biologic organism: (2) its essential functioning is like that of any other organism; (3) it is subjected to the same biological laws; and (4) sociology and economics as sciences must be built on biology.[74]

[73] Sorokin, *op. cit.*, pp. 29–36; also L. I. Petrajitzsky, "Introduction to the Study of Law and Morality," in Russian, *Erlich*, St. Petersburg, 3rd ed., 1908, pp. 65–72.

[74] Sorokin, *op. cit.*, pp. 196–207. *Cf.*, also, R. B. Perry, "General Theory of Value,"

Generally speaking, the shortcomings of organismic analogies are the same as those of mechanistic analogies. They are logically fallacious for similar reasons, and they are not borne out by the facts of experience. But for social theory more can be learned from organismic than from mechanistic analogies, provided we differentiate between two sets of inferences: first (the valid ones) that sociology and economics must to some extent depend on biology, that society is not entirely artificial, and that it represents "a kind of living unity" different from a mere sum of the isolated individuals composing it; second (the invalid ones) that, because of these things, society must itself be an organism and must be actuated by strictly biologic laws.

Society is neither a mechanism nor an organism; but for a

Longmans Green, New York, 1926, pp. 447–452, in which, with delightful irony, he pictures the results of the organismic view of society in the following words:

"It is clear that if the analogy between human society and the cellular composition of the organism is adopted, in so far as an aggregation of men are organized in the image of a man, it must be at the expense of their manhood. If men assume the rôle of cells or organs they must forfeit personal autonomy or delegate it to one of their members. . . .

"In short, the several members of a society can be embraced within one person only in so far as all members save one are subordinated to the purpose which that one has for them. They will enter into the personally integrated society not as interests but as objects. . . .

"A whole is sometimes more highly integrated than its members, as in the relation of the plant to its component cells; and sometimes less integrated, as in the relation of the colony to its component ants. Combining these two types of relationship, we get the case in which an individual whole is both higher than its own constituent members and also higher than the whole of which it is itself a member. The progression from cell to colony is not a steadily ascending progression, as is readily and mistakenly supposed, but an ascent and a descent in which the highest point is reached half-way. This intermediate and highest point, is the animal or man."

"John of Salisbury, in the twelfth century, and Nicholas of Cusa, in the fifteenth, made especially notable attempts to correlate the parts of the 'body natural' and the 'body politic.' According to the former writer, 'the prince is the head, the senate the heart, the court the sides, officers and judges are the eyes, ears and tongue, the executive officials are the unarmed and the army is the armed hand, the financial department is belly and intestines, landfolk, handicraftsmen and the like are the feet.' When these feet are in distress, as is so often the case, the state has gout. The ecclesiastics, using the same method, argued against the claims of the state, that if the emperor as well as the pope were a head, the organism of mankind would be a 'two-headed monster, an animal biceps.' The state, owing to the number of its feet, turned out to be a centipede; and the growing strength of the imperial party forced the conviction that mankind did in fact have two heads. Thus the outcome of an effort to enhance the dignity of men was to conceive them as members or organs of a bicephalous centipede."

constructive study of the phenomena of society, a proper frame of schematic reference is nevertheless helpful. Sorokin outlines four views of social phenomena, the mechanistic, the nominalistic, the organic, and the functional, favoring the last-mentioned since the functional approach is at least free from the pseudo-analogical difficulties inherent in the mechanistic, atomistic (nominalistic), and organismic approaches.[75]

Economic Organismic Systems

We have not thus far dealt with economic organismic analogies as such, even though one of the chief tenets of J. B. Clark's system, which Veblen criticized on other grounds primarily, was organismic in character. Clark's thorough-going use of the organismic analogy is seen in his early work (1886) where he insisted that society is an organism "in literal fact," divided social organisms into "vertebrates, articulates, mollusks, and radiates," spoke of society measuring utilities "like a solitary tenant of an island," stressed the notion of social valuation, and wrote about commodities, like "molecules of nutriment," following the unerring "circulatory laws of the social system" and bringing to each member just what it most needs. Later Clark used both mechanistic and organismic analogies, the first much more than the second; but the latter continued to be in evidence and served to round out the harmonic whole of his system, the idea of "effective social utility" being particularly employed as a presumptive measure of price.[76]

With the transmutation of "cost" into "disutility" went a marked proliferation of the organismic analogy in economic theory. Seligman typified the extent to which the tendency was carried. Value, he said, is primarily expressed in "social marginal utility" on the one hand, and in "social marginal disutility" on the other; these are "real equivalents," no matter how un-

[75] Sorokin, *op. cit.*, pp. 195, 207–213.

[76] J. B. Clark, "The Philosophy of Wealth," *Ginn*, Boston, 1886, pp. 38, 81–83, 86–87; "Distribution of Wealth," *Macmillan*, New York, 1899, pp. 46, 227, 378; "Essentials of Economic Theory," *Macmillan*, New York, 1907. *Cf.*, also, E. R. A. Seligman, "Social Elements in the Theory of Value," *Quarterly Journal of Economics*, May, 1901, pp. 321–347; and B. M. Anderson, Jr., "Social Value," *Houghton Mifflin*, Boston, 1911.

equal individual costs and utilities may be. When cost theory and utility theory are thus "interpreted in their true social light," he went on, they mean the same thing.[77]

There is no need to multiply illustrations of the employment of organicism in economic thinking or to repeat preceding general objections to the organismic analogy. Critical attacks finally came even from utility advocates themselves, from Schumpeter and Davenport outstanding among them.

Schumpeter pointed out that all economists start their analyses with wants and the satisfaction of wants and that this necessarily implies the existence of individuals, for they alone "can feel wants." Society has no sense organs or nervous system. In certain ways, he conceded, social contact naturally influences value, as in imitation and fashion, but beyond such obvious relationships he held that one cannot go without being misled, unless one accepts or takes for granted communistic implications.

Communism and capitalism, he pointed out, represent radically different types of society, with different methods of production, distribution, and valuation. "Social value" as developed through organicism might hold under communism. It does not hold under capitalism.

"Social value," Schumpeter continued, makes for an unwarranted optimism, assumes that each individual and productive agent ultimately gets what he or it is "worth to the community," and sanctions all that is actually happening in society. Only by way of the implications of communism or of a control by a beneficient despot, could the organismic analogy have any practical import. Thus any attempt to substitute "social value" for the actual valuation processes found in modern capitalistic society can lead only to conclusions that are "separated from reality by a great gulf. . . ."[78]

Davenport, like Schumpeter, stressed similar misconceptions but couched his criticism more broadly to cover three historical

[77] Seligman, *op. cit.*, pp. 323–346; also B. M. Anderson, Jr., *op. cit.*

[78] Joseph Schumpeter, "On the Concept of Social Value," *Quarterly Journal of Economics*, February, 1909, pp. 213–232. *Cf.*, also, his "Das Wesen und Hauptinhalt der Theoretischen Nationalökonomie," *Duncker and Humbolt*, Leipzig, 1908.

tendencies making up "a great group or congeries of concurring errors" in economic theory, the Unseen Hand, the Natural Law, and the Laissez Faire doctrines.[79]

The first of these, a belief in the guidance of an Unseen Hand, represents the faith of the religiously devout. Things must be going well, or what can God be about? Each of us is working out the Great Plan; hence the interests of each must be the interests of all, trade is good for the trader and therefore for society, "private gain must accord with public welfare," and each person gets the full product of his social contribution.

Through the second tendency, a belief in the justice of Natural Law in social affairs, the sceptics come to the same conclusions as the devout. It is "the old faith unitarianized," sterilized, and rationalized. Thus there are "great reservoirs of compelling force making for the good"; "the right is destined to ultimate triumph"; "the soul of things is just"; evolution becomes the "last word of scientific faith, and the ameliorative trend a popular certitude."

With the third tendency, a belief in the righteousness of Laissez Faire, Davenport had more patience. Its ascendancy served a better incidental purpose in sweeping away "stupid and unjust legislation," the privileges of "caste and class," and certain obstacles to the "development of popular institutions." But the main result was virtually the same as that of the other two tendencies, and economists used similar arguments: that the good of each is always the good of all, that selfish enterprise results in social welfare, and that past governmental "meddling" has been bad and must be prevented for the future, so that beneficent forces can have unhampered sway.

In the light of these preconceptions, Davenport continued, classical and neo-classical economic theory could not help but be what it is. The conception of competition as "voluntary coöperation," of capital as always socially beneficial, of income

[79] H. J. Davenport, "Social Productivity Versus Private Acquisition," *Quarterly Journal of Economics*, November, 1910, p. 96. *Cf.*, also, his "Cost and Its Significance," *American Economic Review*, Dec., 1911, pp. 724–752, and his "Value and Distribution," *University of Chicago Press*, Chicago, 1908.

as a measure of "social service performed," of distribution as justified by a socially productive process, of all gainful occupation as socially creative,—these and similar conceptions are not confined to any one school of economic theory, but to cost and utility theorists alike, to the "genus economist in general."

Such conceptions, Davenport went on to say, are very far from representing the actual facts, for none of these conclusions is true. "Economic productivity is not a matter of piety or merit or deserving, but only of commanding a price." Similarly with capital. A bawdy house and a burglar's jimmy are capital. "Lobbyists, panderers, and abortionists are producers; that they are paid is the adequate proof." The actual facts are difficult to accept because of the deep-rooted character of our social preconceptions. Hopes and aspirations get constantly mixed with the reality, as though a physician were writing his sympathies into his prescriptions. Until this eighteenth-century background of misapprehension is eliminated from modern thought, "the grotesque unreality of current economic doctrine" will continue, economists will remain busy picturing utopias and insisting that they are real, and polital economy will be nothing more than a "sheer farce."[80]

Davenport's portrayal is very telling and picturesque. False inferences are constantly being drawn from mechanistic, organismic, or other analogies, especially where they serve mainly to keep alive the afore-mentioned theological, naturalistic, or amelioristic presuppositions handed down from the past.

Toward a More Scientific Point of View

We may now focus the considerations presented in preceding pages by contrasting the classical with a more modern point of view and by raising the question as to whether or not neo-classicism can survive.

With respect to human relations, if it be regarded proper to assume a Beneficent Plan, a justice in Natural Law, or a righteousness in Laissez Faire, then all must certainly be "right with the world" and the real and the ideal are identical, at least

[80] Davenport, "Social Productivity Versus Private Acquisition," *op. cit.*, pp. 106–118.

"normally" or in the "long run." God, as an omnipotent, omnipresent, and omniscient Being, is in His Heaven, where He reigns in perfect Absolutism, watches benevolently over the world, and regulates its affairs for the best. On such an assumption, social theory was bound to be for the most part an effort at "rationalization," at making actual conditions appear just, beautiful, and good.

In economic affairs, it seemed equitable to Adam Smith and his followers that goods should exchange according to the quantity of labor embodied in them, according to the relative sacrifices entailed in their production, to the accompanying danger and irksomeness, to the time and waiting that must elapse before productive effort can materialize in finished goods. To the Austrians, it appeared fair that what the consumer pays for goods should measure their usefulness or utility to him. To the hedonists and utilitarians, it seemed proper that the pains or sacrifices of production should equal the pleasures or utilities of consumption. To the rationalists, it appeared reasonable that man should be constantly adding utilities and subtracting disutilities to make sure that the Heavenly Balance was being maintained, though it apparently did not occur to them that they were thereby bringing into question the Divine Bookkeeping, which had previously been assumed to be perfect. In fact, an improvement on the Heavenly Plan was already being proposed, namely, the maximizing of satisfactions and the minimizing of costs, in place of an equilibrated counterpoise between them, surpluses of "utility" rather than a *quid pro quo*. To the mechanists, it seemed fitting that natural laws of a physical nature should be the ones to maintain the balance between production and consumption or, if necessary, to bring the social machine back to its normal "equilibrium of forces." To the organicists, it appeared good and beautiful that society should be an organism, with each part contributing to the organic welfare of the whole according to its productivity and receiving in return the precise equivalent of what it had created or needed, overlooking the fact that production and need are, in actual life, far from being identical.

This "making the wish father to the thought" is, as has been indicated, not a scientific procedure. So long as astronomers, for instance, assumed that heavenly bodies were "perfect" and without "blemish" and that they perforce had to travel in circles, there could be no scientific foundation for their discipline. It took Galileo and Kepler, among others, to destroy this misconception. In preceding pages we have endeavored to show to what extent the classical economic tradition is built on false premises, has made use of illogical reasoning, and fails to accord with the facts of business and of general economic behavior.[81]

Some of the real questions that have not been answered by the traditional theory are the following: What is the history of customary price with respect to various key commodities? How far have habit and consumer ability to pay prevented price from being lowered toward the cost-of-production minimum? In what ways have predatory price-fixing and competition progressed from the stage of "nature red in tooth and claw?" To what extent and in what directions has competition thus far been constantly frustrated by scarcity and monopoly conditions both in production and consumption? What has been the effect of fluctuations in purchasing power upon the maintenance of given standards of living among different classes of society, among business men, property owners, professional men, salaried white-collar workers, unionized skilled laborers, unskilled workers? To what extent are existing standards of living the effect of misleading advertising, high-pressure salesmanship, and the aping of luxurious display, and to what extent do they really reflect "human utility"? These are but a few of the many realistic questions unanswered by traditional economic theory, which will readily suggest themselves in the light of foregoing considerations. . . .

To some minds, however, it will still seem axiomatic that a Beneficent Plan, carried out by an Absolute Deistic Being deal-

[81] The substance of this concluding section, together with a brief epitome of the argument of preceding sections, was presented at the St. Louis meeting of the American Association for the Advancement of Science, January 2, 1936, under the title of "New and Old Economic Doctrines."

ing out earthly Perfection in truth, beauty, and goodness, must be in operation, and that any one questioning such an assumption lacks faith and salvation. Fortunately, however, there has been in the past century a marked change in religious belief and in putative ways to salvation. It is even possible today to be devoutly religious and still to disavow the conception of Deity at the basis of classical preconceptions. Any one, in fact, who is consistently religious and at the same time scientific in a post-Darwinian sense must take a radically different view. For him, Evolution has been substituted for Perfection; Relativity in truth, beauty, and goodness, for Absolutism; Becoming for Being; Immanence for Transcendence; and experiment and control in human affairs for "letting things take their course," which has usually meant letting an unscrupulous minority take advantage of the pious majority. Such a theistic conception is not entirely new. It has been struggling for place, especially since the words of Paul, that "in Him we live and move and have our very being," gave man a claim upon divine inheritance and an inkling of his destiny to help fashion human affairs more in accord with the heart's desire. After the conservative-liberal controversies following 1860, the more modern and scientific point-of-view has taken ever firmer hold until today, as one expression of it in the United States, there is projected a new program of planned democratic control in human affairs, to displace predatory exploitation by small minorities.

That modern business competition is far from being "free," either on the side of demand or supply, is finally coming to be more fully recognized. Competition in matters economic there always was and doubtless always will be. But competition was of a decidedly predatory nature until modern social legislation (representing the forces of community coöperation) stepped in to "free" it somewhat of cave-man influences. Prize fighting was once more predatory than it is today. Marquess of Queensberry rules have helped to make for a more equitable competition there. In a similar sense, coöperative action, in setting the rules of the economic game, lifts competition to a higher plane of freer and more equitable operation. In the economic and social revo-

lution through which the United States now appears to be passing, a laudable effort is being made to formulate a still fairer code of business rules.[81a]

The new emphasis upon human welfare, as against the old emphasis upon the inviolability of vested property holdings, is wholly in line with the dissipation of the misapprehension that utility, cost, and value have economic meaning only in terms of consumption, production, and exchange practices, whatever their character. When deliberate psychological appraisals, which the marginalists erroneously assume to have been constantly made in the past, actually come to be undertaken in any discerning manner, it is found that the real and the ideal are often poles apart and that a normative standard in terms of public benefit is urgently needed to render reasonably significant, terms that otherwise make economic nonsense.

The focusing of attention upon the achievement and maintenance of a fairly invariable purchasing power and a reasonable standard of living for the different groups of a society, rather than upon the encouragement of further wild orgies of frenzied speculation under the guise of business prosperity booms, is another illustration of the more modern point of view.[82] It inferentially repudiates the anachronistic assumption that we still live in a barter economy. As a matter of fact, even business men are not interested in an exchange of goods for goods, but rather in securing a net profit on their transactions; and, in their broader rôle of human beings, they are still less concerned with mechanical swapping operations. They are interested in exchanging their services (given with a maximum of enjoyable effort) for sufficient purchasing power to maintain what they regard as a fair standard of living. The effort is often too painful

[81a] *Cf.*, Edward H. Chamberlin, "The Theory of Monopolistic Competition," *Harvard University Press*, Cambridge, 1933; Joan Robinson, "The Economics of Imperfect Competition," *Macmillan*, New York, 1933; C. J. Ratzlaff, "The Theory of Free Competition," *University of Pennsylvania Press*, Philadelphia, 1936; Arthur R. Burns, "The Decline of Competition", *McGraw-Hill*, New York, 1936; also the bibliographies of Ratzlaff and Burns at the end of their works.

[82] *Cf.*, H. G. Moulton, "The Formation of Capital," *The Brookings Institution*, Washington, D. C., 1935.

and the standard of living attained not always the desirable one. But at any rate these are the exchanges in which human beings are primarily interested—not in a crude, long-since-superseded barter of goods for goods. . . .

Such is the inertia of long-standing social preconceptions, authoritatively promulgated, that in economic thinking, where the classical ideas have taken firmest hold, utility-disutility dogmas are still widely followed, when what we actually need are scientifically-drawn hypotheses about value (based upon an up-to-date knowledge of human nature) and about economic institutions (based upon realistic studies of their actual functioning), such as are already being formulated by a few private bureaus and institutes and latterly by the Federal government itself.[83] As it is, most economists appear to remain content with marginalism and its corollaries, even though some of them recognize that other ways of economic thinking have taken form.

The many die-hards among the classicists and neo-classicists, though perhaps admitting certain logical and psychological shortcomings in cost and utility theory, continue to insist that in the form of marginalism it nevertheless enjoys an indispensable probity and that it will not and should not totally disappear. It is presumed to possess "essential integrity," to bear a considerable "resemblance to the facts," and to be assured permanence through "its pedagogical compactness, its logical coherence and availability, and its large measure of pragmatic truth."[84]

It is one of the contentions of the present article that this "older economics" possesses integrity and is "logical" only on the score of pseudo-scientific assumptions and fallacious preconceptions, that for the most part it bears no resemblance at all to the facts of modern economic life, and that its very availability and pedagogical compactness are a snare rather than a help in the training of future economists except possibly by way

[83] *Cf.*, R. B. Perry, "General Theory of Value," *Longmans Green*, New York, 1926; also, the research activities of the National Bureau of Economic Research, Inc., New York City, and of The Brookings Institution, Washington, D. C.

[84] J. M. Clark, "Recent Developments in Economics," in E. C. Hayes' "Recent Developments in the Social Sciences," *Lippincott*, Philadelphia, 1927, pp. 253, 305–306.

of an object lesson in fallacious dialectic. Should this appraisal be correct, even though certain historical achievements in economic analysis may be retained in their proper setting and some of them utilized in the projection of more adequate hypotheses, the classical structure as such, together with its neo-classical refinements, must be discarded in its entirety if a truly scientific approach to the problems of modern economic society is to be achieved.

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REVIEWS AND NOTES

Le développement du Cercle de Vienne et l'avenir de l'empirisme logique. By Dr. Otto Neurath. Paris: Hermann et Cie., 1935. Pp. 59.

This monograph is published in the series *Actualités scientifiques et industrielles*, which contains in translation a number of writings by members of the *Wiener Kreis* and related groups (Carnap, Schlick, Frank, Reichenbach). The present volume by Neurath gives the first fairly comprehensive historical and sociological survey of the factors which have culminated in the present international movement known by such names as "the unity of science movement," "logical positivism," "scientific empiricism." It traces especially the place which the *Wiener Kreis* has played in the development.

The monograph traces the way in which Austria came to be an especially fruitful soil for the cross-fertilization of the emphasis upon logical analysis with the emphasis upon a generalized empirical point of view. Austria had avoided for the most part the Kantian and idealistic movements which had dominated Germany. On the contrary it had received from England and France a strong dose of eighteenth century utilitarianism, liberalism, and empiricism; it had retained direct connection with the tradition of Leibniz; and from its Catholic background it had gained a persistent interest in logical analysis. The author makes the point that the movement of logical positivism has developed most rapidly in countries of a Catholic tradition; it has not developed so vigorously in Protestant countries such as Germany—there is today, for instance, only one professorship for symbolic logic in the whole of Germany. The Catholic background seems on the one hand to have provided a natural point

of view for the logical analysis of the structure of science, and on the other hand to have necessitated a sharp break with metaphysics when the break did appear. In contrast, the elastic thought of Protestant countries shows a tendency to stretch and adapt metaphysical categories rather than desert them, and the stress on metaphysical adaptation was accompanied by a neglect of logic or the transformation of logic itself into metaphysics. From what has been said it is evident that Neurath's approach is from the point of view of a sociology of science; the book does, however, give a perspective upon the general orientation of logical positivism and contains some wise warnings against absolutism and excessive formalism within the unity of science movement itself.

Dr. Neurath does not in the monograph make clear the part which he has played in the later phases of the development which he traces. This is, however, indicated in the (unsigned) preface by Rougier. Neurath has been a central figure in the formation of the doctrine of physicalism, he has directed logical studies to the field of the social sciences, he has been the active center in the organization of the *International Congresses for the Unity of Science*, and he is now to act as general editor of the *International Encyclopaedia of Unified Science*. This work will exhibit the present status of the now genuinely international unity of science movement, a movement within which the *Wiener Kreis* has performed a distinguished rôle.

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CHANCE IS REAL

A World of Chance. By Edward Gleason Spaulding. The Macmillan Co., New York, 1935. Pp. xxxiii + 293.

This book, by the McCosh Professor of Philosophy at Princeton University, is a further development of the "new realism" with which the author has been identified. Based on a pleuralistic metaphysics and emphasizing relativity and indeterminism in the larger scheme of things, Professor Spaulding's book presents a synthesis of the results of research in symbolic logic, the physical

sciences, emergent evolution, and theories of knowledge and values. Since the volume for the most part is a defense of the thesis that ontological contingency is a logical necessity, the answers to the questions, *Whence*, *Whither*, and *Why*? are these: *From no Source*, *To no End*, and *For no Reason*. This "structure" of reality, shot through with chance and indeterminism, is said to provide a place for Autonomy and Freedom, and for Postulational Method and Rational Inquiry.

Professor Spaulding's book is "different" because he uses the technique of mathematical logic in handling philosophical problems. While this is what gives the book its peculiar interest, it is perhaps also the thing which makes it most easily subject to criticism. Aside from the possibility of complications arising out of his hierarchy of concepts and logical machinery, there is a fundamental epistemological problem connected with the observation of E. T. Bell that "no satisfactory justification has ever been given for connecting in any way the consequences of mathematical reasoning with the physical world." To be sure, Professor Spaulding's realism of "external relations" claims to bridge this epistemological chasm. To the reviewer, however, it does not seem that he has successfully made the leap from "thoughts" to "things." The only theory I am aware of which can close the gap is to be found in the idea of light as the link between logic and physics. This theory pretends to solve the epistemological problem by enabling us to infer the structure of thought from the physical structure of light, and the structure of physical reality from the structure of thought. Such a view seems preferable to the intellectual legerdermain involved in Spaulding's attempted validation of knowledge (p. 213) in the face of that interpretation of Indeterminacy which asserts that nothing can be observed as it really is because observation disturbs that which is observed. Spaulding refutes the defenders of this view (of whom he names Dewey) by arguing that if observation disturbs that which is observed, then this fact is in turn the "observed" to another observation, and so on indefinitely. The outcome is that sooner or later there is an observation of some order or type which does not disturb that which is observed—so Spaulding says.

Aristotle arrived at God through a refusal to accept an infinite regress, and Professor Spaulding arrives at knowledge through the same strategy. If we accept the above mentioned theory of light, logic and physics, we can accept the "photocentric predicament" and the "logocentric predicament" as two phases of the same situation and get knowledge regardless of the outcome of the dubious problem of whether an observation of an observation leads to a regress which "sooner or later" must come to a stop.

So much by way of personal reaction. Whatever one may think of the positive outcome of Professor Spaulding's philosophy, it must be recognized that the "World of Chance" constitutes a highly significant contribution to present-day philosophy.

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ARISTOTLE, GALILEO AND THE LEANING TOWERS OF SCIENCE

Principles of Topological Psychology. By Kurt Lewin. McGraw-Hill Book Co., New York, 1936. Pp. ix + 231. $2.50.

Professor Köhler once was asked to explain the differences between behaviorism and gestalt psychology. His immediate comeback was: "Whose behaviorism"? This amused the audience and incited a then-present behaviorist, Dr. A. P. Weiss, to exclaim: "Whose gestaltism"? Since those days this query has become both more and less appropriate. In the present book, dedicated TO THE HEBREW UNIVERSITY, Jerusalem, the Preface consists of an open letter to Dr. Wolfgang Köhler, in which Lewin states, "I have tried my best to destroy the myth that Gestaltists do not attack each other." It is devoutly to be hoped that this will not lead to any diplomatic crisis. If a "league of notions" dating back to the intellectual "collective" in Berlin can survive Lewin's innovations and tentative overtures to the formerly hostile camp of logical positivism, surely there is still hope for a league of nations working out amicable adjustments in a political world full of "boundaries," "tensions," "barriers," "vectors," and the like.

This last sentence takes us directly into the subject of topology, for the terminology descriptive of individual psychology is also applicable to social groups. Lewin states that the fundamental task of all psychological problems is the determination of topological relationships. Topology—for those who are not acquainted with the subject—is the most general (non-metrical) science of spacial relationships. As such, it is concerned with *whole-part* relations, and with the cognate concepts of *surrounding region*, *point*, *being-included-in*, *Jordan curve*, etc. These and related ideas have to do with the topological structure of a region as determining the possible locomotions in that space. That is, topological psychology assigns a separate space to each individual and his environment, and this *life space* corresponds to the psychological locomotions which can be carried out. Thus Lewin is convinced that spacial concepts of a strictly mathematical nature, when given content through such notions as "solidity of boundaries," "fluidity of materials," "fields of force," etc., can be profitably employed in the representation of psychological facts. This inevitably leads to a consideration of such interesting questions as, How many dimensions are needed for the expression of psychological facts? What is the relation between the life space of psychology, the phase space of physics, the n-dimensional space of mathematics?, and so on.

In the present work the earlier distinctions between the Aristotelian and Galilean modes of thinking are again operationally active. Lewin, for example, states that in handling a psychological phenomenon the Aristotelian way of thinking explains the fact in terms of the individual as such, from an inner drive or emotion; whereas the Galilean view demands that we no longer seek the "cause" of events in the nature of a single isolated object, but in the relationship between an object and its surroundings. Thus human behavior is always a function of the person in an environment: $B = f(P, E)$. The older question of whether heredity or environment plays the greater rôle belongs to the pre-Galilean or Aristotelian mode of thought. Now modern psychology (Galilean and topological) provides us with methods of representing persons and environments in common terms as parts of one "situation."

At this point one begins to wonder whether Lewin is not placing an arbitrary limitation upon the development of psychology. If psychology is now in the position of physics at the time of Galileo and Newton, and if—as we know—Newtonian physics was succeeded by post-Newtonian (Einsteinian) physics, is psychology likewise to outgrow its Galilean chrysalis and metamorphose into a post-Galilean phase? If so, why may we not take a short-cut and jump directly into the post-Galilean phase? As a matter of fact, wouldn't the more general development of a non-Aristotelian science include and transcend the Galilean revolution? That gestalt psychology will eventually have to adopt this non-Aristotelian approach is suggested by the reviewer (in an article on Non-Aristotelian Logic in the *Monist*, July, 1936). This is so because the relativity of "wholes" vitiates the *law of identity* insofar as it is connected with the Aristotelian fallacy of the absolute individuality of substance (subject of predication). This point was glimpsed by George Hartman in his book on Gestalt Psychology, and he informs me that Gestalters would probably reply to the criticism by maintaining that true "identity" applies only to genuine "wholes" and not to the "parts." This view has also been stated in one form by three defenders of gestalt theory (Wheeler, Perkins and Bartley, *Psychol. Rev.*, Vol. 38, 1931, p. 116) as follows: "There is no such thing as relative unity; unity is absolute with respect to any given set of conditions; and as conditions change, wholes change phenomenologically, not functionally." Professor Lewin probably intends the same thought when (p. 172), in referring to dynamic unities of the highest degree, he says, "They are regions whose parts are so closely connected that each change of one part results in change of all other parts. These regions are therefore dynamic unities or gestalten in the sense in which Köhler uses the word. . . . The unity of the regions is therefore, in spite of its relativity, objective in character."

Now this may be a defensible psychological doctrine, but if one also pretends to assimilate physics and psychology difficulties immediately arise. Relativity theory (post-Galilean!) teaches us that *forms* are relative, e.g., what is a straight line in one frame

of reference is a curve in another, and a circle is (becomes) an ellipse when viewed from a frame moving at right angles to the "circle." This merely restates the difficulty which the reviewer points out in connection with Koffka's latest book (*Phil. Rev.*, July, 1936). Gestalt theory splits the four-dimensional manifold of relativity by the virtual reintroduction of a privileged "local" time of the ego-frame. There are ways out of this difficulty, but since, as Emile Meyerson points out, the irreversibility of time must remain an "irrational" in the symmetrical space-time manifold, the escape is not to be attained through relativity theory.

Up to the present philosophers have remained somewhat in ignorance of the Galilean school of psychology. But as soon as the modern followers of Aristotle come across the comparison between the Aristotelian and Galilean modes of thought—to the detriment of Aristotle—the fight will be on! Thus one reviewer of Lan Cooper's book on *Aristotle, Galileo, and the Tower of Pisa* (J. H. Randall, Jr. in *J. Philos.*, Vol. 32, 1935, p. 584) makes the point that "the father of modern science turns out to be none other than the master of them that know." Evidently Professor Lewin and those who sympathize with his un-Aristotelian views will be in for an interesting time when the Aristotelians get wind of these heresies.

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